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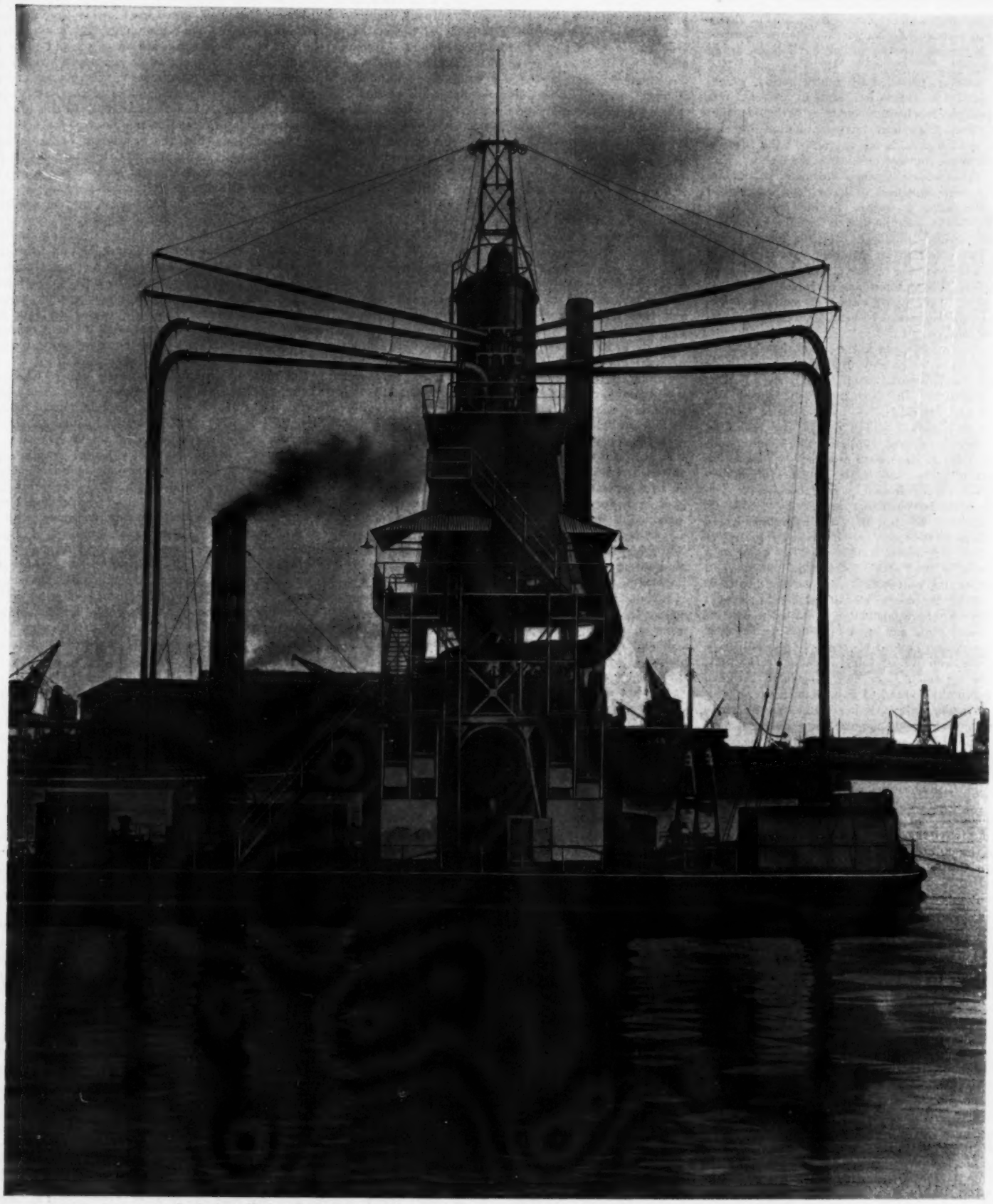


Grain installation fixed on a floating pontoon, recently placed in service at Emden on the North Sea.

PNEUMATIC GRAIN ELEVATORS.—[See page 388.]

Theories of Ore-Genesis*

Past and Current Views Considered

By Dr. F. H. Hatch

THE subject of ore-genesis is of the greatest importance to the mining engineer, for it is evident that every forecast of the continuity of an ore-body beyond the limits of the ore in sight must, if it is not to be entirely empirical, rest on some hypothesis as to origin. This field of inquiry has since the beginning of this, and during the latter half of the past century, riveted the attention of the best mining geologists in all parts of the world. In a comparatively small interval of time, our knowledge has advanced by leaps and bounds, and many important principles governing ore-deposition have been firmly established.

It was, however, preceded by a long period, which, although fertile in suggestion and hypothesis, was not one of real progress because, contrary to the Baconian principle, "Non fingendum aut cogitandum sed inveniendum quid natura faciat aut ferat," the theories advanced were not founded on ascertained facts.

Prior to the sixteenth century the metallic contents of ore-veins were supposed to have been determined by their orientation in regard to the planets; and Agricola (1494-1555) was the first to formulate a reasonable genetic theory. Reduced to its simplest terms, Agricola's view was that ore-channels (*canales*), formed by erosion, had been filled by metallic minerals deposited from solution. These solutions, or juices (*succi*), as Agricola terms them, were waters of meteoric origin which, under the influence of heat, had taken mineral matter into solution.

From the time of Agricola to the end of the eighteenth century, the mines of Saxony produced nearly all the writers on vein-formation. Such were Rösler, Becher, Henckel, Hoffmann, Zimmermann, von Oppel, von Charpentier and von Treba.

Becher and Henckel, who wrote in the beginning of the eighteenth century, supposed the metallic constituents of veins to have been produced by the action on pre-existing stony and earthy matters of subterranean vapors arising from certain processes of "fermentation" in the bowels of the earth.

In 1749 Zimmermann put forward a hypothesis which clearly had in it the germ of the modern theory of metasomatism. He ascribed the origin of veins to a transformation of the rocks into metallic minerals and their accompanying vein-stones, along certain directions now marked by the course of the veins, the solvents that effected the alteration finding a path through innumerable small rents and other openings in the rocks.

But Zimmermann applied his theory indiscriminately to explain the origin of all veins, including those that, by common agreement, are now considered to have been formed by the filling of fissures without replacement. Von Treba, in supporting Zimmermann's view, insisted particularly on the far-reaching changes effected in rocks by circulating waters, especially when aided by heat. "I am persuaded," he wrote in 1785, "that there is constantly going on in our mountains a variety of transformations, compositions and decompositions, which not only take place at present, but will continue to the end of time."

According to Gerhard, who wrote in 1781, waters circulating through the rocks adjacent to a vein become charged with certain of the metallic and earthy substances contained in them. Passing through the crevices and interstices of the rocks to the larger rents and fractures, they deposit their mineral burden in cavities which, when filled, become veins. It will be seen that Gerhard's hypothesis must be regarded as a precursor of the more modern theory of *lateral secretion*.

To von Oppel belongs the credit of having shown that mineral veins were largely the filling of fault-fissures, a principle which up to that time does not appear to have been clearly recognized.

At the end of the eighteenth century the mining world was dominated in all matters relating to ore-genesis by the famous Freiberg professor, Abraham Gottlieb Werner, who insisted that all veins, including those that we now term "intrusive dykes," had resulted from the filling of contraction-fissures open above and connected with the primeval universal ocean, which, according to the Wernerian doctrine, covered the globe and contained in solution all the necessary materials for the formation of its crust. These waters, descending into the fissures from above, deposited the vein minerals by chemical precipitation.

This Neptunist view was in the beginning of the nineteenth century attacked and finally overcome by Hutton and his Plutonist or Vulcanist school. Unfortunately, however, the Plutonists went to the other extreme, and would not allow even ore-veins to have any other than an igneous origin: "The materials," wrote Playfair, "which fill the mineral veins were melted by heat and forcibly injected into the clefts and fissures of the strata."

But Hutton's broad generalization, even with the important modifications of Elie de Beaumont, Daubrée and Durocher to the effect that many of the metallic ores had been deposited from vapors and solutions emanating from cooling igneous magmas, was soon discarded in favor of the deposition from waters of meteoric origin; and an animated discussion was maintained for half a century on the respective merits of the *descensionist*, *ascensionist* and *lateral secretionist* theories; or, in other words, whether the mineral burden of the circulating waters instrumental in vein-formation was derived from superficial rocks, from deep-seated sources, or from the wall-rocks of the veins themselves.

The chief supporters of the modified form of the ascension theory here alluded to, which must, of course, be distinguished from de Beaumont's *ascension by emanation*, were Stelzner and Posepny. They argued that the ground-water (originating by precipitation from the atmosphere) descends by capillarity through the interstices of the rocks to deep-seated regions, and thus acquires a high temperature and pressure, and, consequently, a vastly increased solvent power, whereby in its passage through the rocks it is enabled to take up certain of the mineral substances there disseminated in a minute form. At a certain depth the water moves laterally toward open conduits, on reaching which it ascends toward the surface, depositing its mineral burden in proportion to the decrease of temperature and pressure.

It has been seen that the theory of lateral secretion, or the derivation of the mineral contents of veins by an aqueous leaching of the country rock, was advanced in a crude form as early as 1781 by Gerhard; but it remained a mere hypothesis without the support of ascertained facts until the middle of the nineteenth century, when the chemical work of Bischof, Forchhammer and Sandberger definitely established two important facts in support of the theory, namely—(1) That the gangue of ore-veins varies in correspondence with the wall-rock; and (2) that the heavy metals occur in minute traces in certain of the igneous and sedimentary rocks constituting the "country" of ore-veins.

Sandberger's researches were specially directed to prove that the heavy metals (gold, silver, copper, lead, etc.) are contained in the common ferro-magnesian silicates (namely, the micas, hornblendes and augites) of the igneous rocks; and having satisfied himself on this point he was led to extend his investigations to the sedimentary rocks, with the result that small quantities of the heavy metals were found in the sediments of all ages, and especially in the slates of the older systems. Whether, however, they are there present as constituents of sporadic fragments of ferro-magnesian silicates derived from igneous rocks, or as sulphides that were introduced during the secondary mineralization connected with ore-deposition, was not satisfactorily settled by Sandberger's researches. The more recent work of Don, carried out on a great variety of material, tends to show that the ferro-magnesian silicates do not carry gold or silver in amounts determinable by chemical analysis. Where the rocks examined by him were found to contain these metals they were present as a constituent of sulphides, such as iron pyrites, pyrrhotite, mispickel, chalcopyrite and galena, which in most cases are secondary introductions.

But long before this the inapplicability of lateral secretion as Sandberger conceived it had become apparent; and the theory became the subject of vigorous attack on the part of Stelzner and Posepny.

Lateral secretion, in a much more extended sense and in combination with the ascension theory, is advocated by Van Hise. Van Hise's view may be briefly summarized thus: The meteoric waters, after penetrating the surface, are widely scattered through the rocks in innumerable small openings as they travel downward to great depths in the earth's crust. With steadily increasing temperature and pressure they take up mineral matter. The downward movement ultimately develops a lateral component, by which the waters are carried to the larger openings. During this process, also, the waters continue to take material into solution. In the larger openings the waters ascend with decreasing temperature and pressure, and there the ores are deposited.

It will be seen that this view is a combination of the ascension and the lateral secretion theories, and presupposes the existence of a continuous sheet of water in circulation between the ground-water level and the lower limit of the "zone of fracture," no circulation being admittedly possible in the underlying "zone of flowage." The weak point in Van Hise's assumption of a "sea of underground water" lies in the fact that deep mines are usually found to be dry, the drainage being confined to the upper levels This, for instance, is the experience in the copper mines of Lake Superior, in the gold mines of the Rand, and in those of Bendigo. Van Hise, in reply to this criticism, attributes this dry zone to the closing of the passages by cementation; but the restriction of the ground-water circulation is equally fatal to the meteoric derivation of deep-seated thermal springs and other phenomena connected with vulcanicity.

In recent years there has been a partial reaction to igneous views. Thus certain classes of ore-deposits are now held to have been formed by a *differentiation of igneous magmas* prior to consolidation. Such, for instance, is the origin ascribed to certain titaniferous iron-ores in basic eruptives, chromite in peridotites, nickeliferous pyrrhotite in norite and primary platinum in ultra-basic rocks. Similarly pegmatites, and even some quartz-veins, are considered to have originated by the consolidation of the aqueo-siliceous residuum of a slowly cooling granite magma.

But more important in its application to ore-deposition than *magmatic differentiation* is the theory which Vogt has founded on the metalliferous emanation hypothesis, by which Elie de Beaumont and Daubrée sought to explain the origin of tin-ore deposits. According to the *pneumatolytic* theory, certain *agents minéralisateurs*, such as fluorine, chlorine, sulphur, phosphorus, silicon and boron, have the property of forming with the metals volatile compounds, which escape from the granite-magma as gases with low critical temperatures (the *aura granitica* of Elie de Beaumont). These compounds ascend through already formed fissures in the overlying rocks, or force their own passage by attacking the minerals that compose them. In this manner, for instance, cassiterite, wolfram, tourmaline, fluorspar, topaz, beryl, axinite, datolite, apatite, etc., are deposited either in the granite itself, or in the sediments comprised within its metamorphic aureole.

Closely connected with pneumatolysis is the rôle ascribed in ore-deposition to the so-called *magmatic waters*, a term that has come into use for water not of atmospheric origin, but dissolved or occluded in some way in molten magmas, from which it separates by liquation and distillation on the fall of temperature and pressure. In it are concentrated the substances that (at the existing temperature) are more soluble in water than in the silicate magma.

Suess, in an address on the Karlsbad springs, delivered in 1902, directed attention to the connection existing between thermal springs, vulcanicity and ore-deposition. He applied the term *hypogene* or *juvenile* to thermal springs (like those of Karlsbad) which, originating in the depths of the earth's crust, bring water to the surface for the first time. Such hot springs are, in fact, the last survivors of vulcanicity, being the relics of a late stage of fumarole activity. Their mineral content comprises readily soluble compounds of the alkalies and alkaline earths, together with, and partly in combination with, sulphur, chlorine and carbon dioxide, the less soluble metallic compounds having already been deposited as ores at lower depths in the earth's crust. According to Suess the after-products of eruption vary with the temperature; in the earlier (pneumatolytic) *phases of emanation* the gases are dry and their deposits (such as tin-ore and its accompanying boron, fluorine, tungsten and uranium minerals) are the products of sublimation. At a later period, strongly alkaline magmatic waters are given off, and to these are attributable the *sulphide* and *arsenide phases* of vein-formation, e. g., the deposition of iron pyrites, chalcopyrite, primary bornite and chalcocite, enargite, galena, blende, etc.

But although, as we have seen, waters of meteoric origin have been displaced from their pride of place as agents of deposition for what we must term the primary sulphide ores, they are undoubtedly the formative agents for a considerable number of ore-deposits, including the products of oxidation, chlorination and

* From the presidential address delivered at the annual meeting of the Institution of Mining and Metallurgy, and reported in *Nature*.

reduction above the permanent water level, and the secondarily enriched ores usually found immediately below the junction of the zone of oxidation with the zone of primary sulphides. So important are the functions of the vadose waters (to use Posepny's term for the shallow water circulation) in dissolving and redepositing at a lower level the ores of copper in a concentrated form, that it has been confidently stated that the bulk of the copper production of the world, not alone in the past, but also at the present time, is drawn from the zone of secondary enrichment. This view will, perhaps, appear exaggerated in the light of the results recently obtained by Sales at Butte; but for the majority of the great copper deposits of the world it may still pass unchallenged.

In the same way, vast deposits of high-grade iron-ore have been formed as the result of secondary enrichment, but under entirely different conditions from those that determine copper-ore enrichment. Thus the hematite ores of Lake Superior are believed by Van Hise to have been derived by the oxidizing and concentrating action of vadose waters, from a low-grade cherty iron carbonate originally deposited under water as a chemical sediment; and he draws the important conclusion that "the ore-bodies cannot be expected to extend beyond the depth to which the descending waters may bear oxygen and precipitate iron oxide." He has "no doubt that vastly more high-grade iron-ore will be taken out in the Lake Superior region above the 1,000-foot level than below it." If this be true, the iron-ores of that district, with more than 60 per cent of metallic iron, are not inexhaustible.

The enrichment of gold-ores also takes place in the zone of oxidation, but in their case the action of the vadose waters results in an abstraction of the more soluble and less valuable metals, leaving behind a smaller quantity but a relatively richer material; in other words, a diminution of the specific gravity of the whole material raises the gold tenor. Furthermore, there is also an increase in the fineness of the gold, due to the removal of a portion of the silver with which it is alloyed. The Mount Morgan mine in Queensland is a good instance of a gold-ore enrichment brought by the vadose circulation; here the oxidation of a pyritic copper lode with subordinate gold has, by the removal of the sulphides of iron and copper, led to the formation of an upper zone of cellular quartz, in which the increased ratio of gold to vein-stuff was the *vera causa* of the richness of one of the premier gold mines of the world. But, as with increasing depth the mine-workings are extended below the oxidation-zone, the copper production is becoming more important than the gold yield.

The Witwatersrand Banket is another example. In this case the primary ore is auriferous iron pyrites disseminated in a quartz-conglomerate on which intense silicification during cementation has impressed the character of a quartz vein. The removal of the pyrites from the zone of oxidation, which extends to 200–300 feet below the surface, left an enriched free-milling ore that gave marvelous returns on the amalgamation plates of the first Rand mills. Since the exhaustion of this high-grade, free-milling ore early in the history of gold mining on the Rand, the mines have been worked in low-grade unoxidized pyritic ore; and this has shown a gradual but steady impoverishment with increasing depth—a fact which supports the view that the gold of this deposit was precipitated by ascending thermal waters in proportion to their loss of temperature and pressure.

One of the most remarkable advances in the science of ore-genesis during the period under review is the recognition of the important rôle played by metasomatism in the formation of ore-bodies. The fact that the rocks adjacent to vein-fillings often contain small quantities of metallic ores similar to those composing the veins themselves, or are altered for some distance away from them, was observed at an early date, but its significance was very differently interpreted. The lateral secretionists pointed on one hand to ore-disseminations in the wall-rocks as indicating the source of the vein-filling, and on the other, to the alteration of these rocks as a concomitant of the leaching that collected the filling material. But the ascensionists, whether belonging to the school of Elie de Beaumont and Durocher or to that of Stelzner and Posepny, recognized that the solutions from which the materials of the lode were precipitated, whether gaseous or liquid, also penetrated the walls and there caused certain deposits in the rock itself—metallic ores taking the place of some other mineral dissolved, as, for example, when cassiterite forms pseudomorphs after feldspar in the granite country of tin-veins. Much evidence favoring the latter view has since been accumulated. Thus Posepny described in 1873 the replacement of carbonate of calcium by carbonate of zinc in the Raibl deposits; and Pumpelly in the same year attributed the origin of the native copper of the famous Michigan deposits to metasomatic processes.

CLASSIFICATION OF ORE DEPOSITS.

Nature of Deposit.	Vehicle or Agent of Ore-Deposition.					
	a. Molten Magmas.	*b.* Gases and Vapors above their critical temperatures.	*c.* Deep-seated waters, whether of magmatic or meteoric origin.	*d.* Vadose Waters.	*e.* Mechanical Agents such as moving water and wind.	*f.* Chemical and Bacterial Agents in seas, lakes and swamps.
1. IGNEOUS DIFFERENTIATES.	*Certain Massive Iron and Nickel Ores associated with basic igneous intrusions* (e. g., those of Sudbury in Ontario).					
2. CAVITY-FILLINGS.	*Injected Tin-Ores* (e. g., tin-pegmatites and tin-elvans on the margin of granite intrusions).	*Pneumatolytic Cavity-fillings* (e. g., tin quartz veins).	*Hydato-genetic Cavity-fillings* (many fissure veins).	*Superficial Fracture-fillings*, such as gash-veins in limestones and cavity fillings (e. g., the hematite-ores of Cumberland).		
3. METASOMATIC REPLACEMENTS.		*Pneumatolytic Replacements* (e. g., tin-greisens and many contact-deposits).	*Hydato-genetic Replacements.* (Many veins and massive deposits, also the Rand Banket).	*Some Lead and Zinc Ores in limestones. Iron-ores replacing limestones* (e. g., Cleveland). *Some lateritic iron and manganese deposits. Secondary enrichments of copper ores.*		
4. STRATIFIED DEPOSITS.			*Possibly some Sedimentary Deposits in which the cementing materials are ores of the metals.*	*Some Lead and Copper Ores interstitial in sandstones and shales.*	*Mechanical Concentrates in bedded deposits* (e.g., gold and platinum placers, stream-tin, iron-sands, detrital laterites and other metalliferous gravels and sands).	*Chemical and Bacterial Sediments* (e. g., lake- and bog-iron-ores; clay-ironstone and other sedimentary siderites; bog-manganese-ore and other sedimentary manganese ores).
5. RESIDUAL DEPOSITS.				*Mantle-deposits* e. g., pisolitic and nodular ores of iron (Bilbao and Appalachian hematites and limonites of manganese (psilomelane) and of aluminium (bauxite).	*Eluvial Gravels formed near the outcrop of veins* (e. g., those of gold, cassiterite and wolfram, galena and zinc-ores).	

In 1881 Emmons showed that the Leadville silver-lead deposits had been formed by the replacement of limestone by galena, blende and pyrites, an alteration which, although chemically complete, left untouched the granular texture, joints and other structural features of the original limestone. He pointed out that the resemblance of the altered rock to limestone was so perfect that, when the faces of the drifts were covered with dust, the observer was often completely deceived until the breaking of a fresh fragment with the hammer revealed the metallic gleam of galena beneath.

In a later paper he showed that many so-called fissure veins were not true cavity-fillings, but owed their origin to the metasomatic replacement of the rock material by substances brought in by solutions circulating along fault-fissures, through crush-zones or in sheeted zones. In such cases a vein may be formed by the replacement of the material enclosed between adjacent parallel fractures, true cavity-filling being only of a restricted character. Owing to the difference in character between replaced sheets of country rock and the filling of the fissures that divide them, deposits formed in this way sometimes possess a banded structure, which, however, is distinguishable from the normal "crustification" of vein-fillings. The whole subject has been admirably reviewed by Lindgren and by J. D. Irving in their well-known papers, in which will be found many illustrations of the potency of metasomatic processes in vein-formation.

Modern views on ore-genesis may be reduced to two principal lines of inquiry, one dealing with the agent or vehicle by which the metals have been collected, conveyed to and deposited in the places where they are now found, and the other with the nature of the concentrates formed in the course of these processes.

Considering the latter first, ore-deposits are found to be either: (1) Igneous differentiates; (2) cavity-fillings; (3) metasomatic replacements; (4) stratified or sedimentary deposits; (5) residual deposits. Of these, the sedimentary deposits comprise marine, lacustrine and fluviatile accumulations, including placers.

Coming now to the agents or vehicles of ore-concentration, these are found to be: (*a*) Molten magmas; (*b*) gases and vapors above their critical temperature; (*c*) deep-seated waters, whether of magmatic or of meteoric origin; (*d*) vadose waters; (*e*) chemical and bacterial agents in lakes and seas; (*f*) mechanical agents, such as moving water and wind.

It is possible, by combining the facts elicited by these two lines of inquiry, to formulate a genetic scheme of classification. For example, cavity-filling may be due to gaseous injection, to gases and vapors above their critical temperatures, to deep-seated waters or to vadose waters; again, metasomatic replacement may be brought about by gases and vapors, by deep-seated waters or by vadose waters. By arranging these two series of relationships in vertical and horizontal columns respectively, all the various types of ore-deposits are obtained at their intersections, and in this way the classification shown in the table is obtained.

The Vulcanization of Rubber by the Ultra-Violet Rays

Prof. Dastre, member of the Academy of Sciences, in the names of Messrs. André Helbronner and Gustave Bernstein, has communicated a paper concerning the vulcanization of rubber solutions by ultra-violet rays. The action of these rays on solutions of rubber to which sulphur has been added produces a combination between these two bodies without, however, the rubber, although vulcanized, being precipitated from the solutions. Contrary to what might be expected, remarkably stable solutions are thus obtained, and these solutions give, by evaporation, a perfectly insoluble pellicule. The analysis of the phenomenon enables one to follow, step by step, the process of vulcanization. The ultra-violet rays do, indeed, precipitate the sulphur and depolymerize the rubber. These are two concomitant actions, combined with the repolymerization of the rubber under the action of the sulphur, which constitute the vulcanization of which the intimate nature is thus revealed, and completely confirm the theories already expressed by one of the authors. If we pass now to the practical side of the matter, the solutions vulcanized by the ultra-violet rays have an interesting future before them. They enable all kinds of repairs to be effected for any object in rubber. A veritable autogenous soldering is thus obtained of this matter, resisting heat or any other mechanical action, and it thus becomes unnecessary to perform an ulterior vulcanization, which long and delicate operation was before obligatory. It is then quite a little revolution for all repairs, and especially that of rubber tyres. This interesting discovery will also have most important applications in the industry of rubber clothes, of envelopes of dirigible balloons, of shoes, etc.—*Chemical News.*

Protecting the Engine and the Aviator.—Stanislaw Mroczkowski, of Naticoke, Pa., in patent No. 1,096,639, provides adjustable shades or curtains for protecting the engine when the aeroplane is not in use and also to protect the aviator and engine when the weather is inclement.

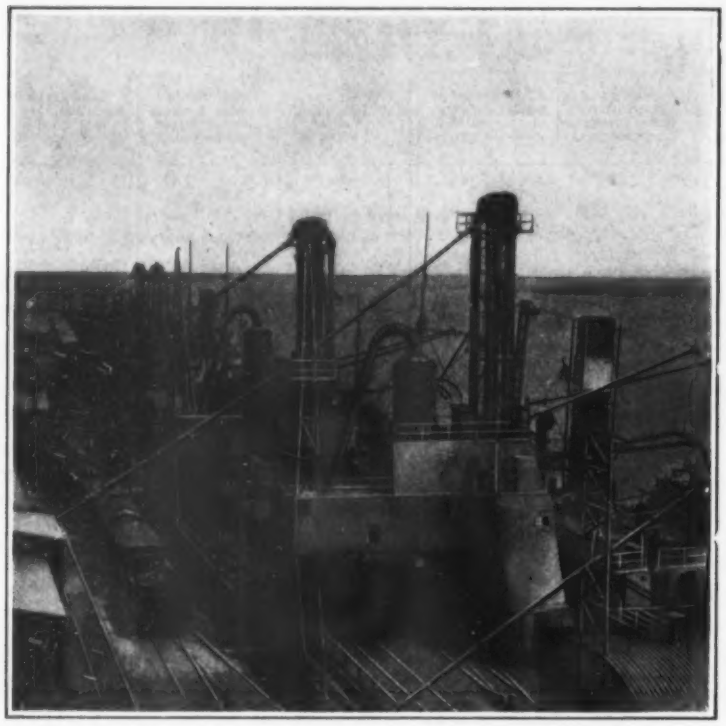


Fig. 1.—The pneumatic grain elevator installation at Brake on the Weser (Germany).

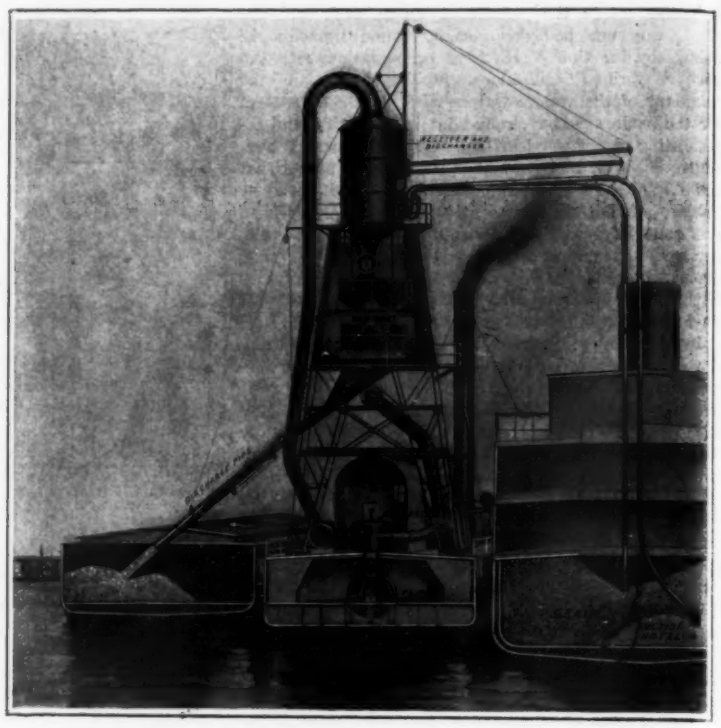


Fig. 2.—Sectional elevation of floating pneumatic grain elevator at work.

Pneumatic Grain Elevators*

The Ideal Method of Handling Cargoes

By Jacques Boyer

It has become a well-established custom, in handling grain in large quantities, to use bucket-chain elevators for vertical transfer, and worm conveyers or traveling

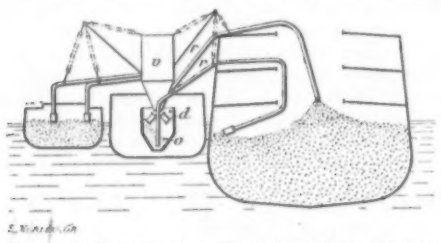


Fig. 3.—Diagram of pneumatic grain conveyer.

belts for horizontal motion. By such means as this, a large cargo can be loaded or unloaded in a very short time and with but small expenditure of power, even if the vessel is anchored at some distance from the store of grain.

But apparatus of the kind mentioned presents one disadvantage: If any change is to be made in the path of travel of the material handled, it becomes necessary to introduce more or less complicated intermediate apparatus, and the natural simplicity of the installation is therewith forfeited. In fact, the handling of grain cargoes in port presents technical difficulties which vary according to the size of the ships, the location of the cargo, etc. Thus, on first starting to unload, the grain reaches right up to the hatchway, say 15 to 20 feet above water-level, so that the bottom of the elevator must be set at that elevation. Later, as the cargo dwindles down, the elevator must dig deeper and deeper, ultimately, some 20 feet below the water-level. Finally, when the cargo is completely unloaded, it becomes necessary to lift the end of the elevator over the deck, say 40 feet above water-level. In other words, the end of the elevator must be capable of an extreme travel over some 60 feet up and down, or more in the case of large modern transatlantic steamers.

In view of these difficulties, the older methods, using bucket chains, etc., have been largely abandoned in favor of pneumatic grain conveyers. These consume a little more power than the older types, but offer great advantages in point of ease of handling, economy of space, improvement of the merchandise (since dust is automatically separated from the grain), etc.

One of the most recent types of pneumatic grain elevators is constructed by a German firm of Brunswick, on a principle diagrammatically indicated in Fig. 3. In this drawing, v represents a vacuum chamber, kept exhausted by means of vertical or horizontal piston pumps. Air is thus sucked in through the pipes r and draws with it the grain from the hold of the steamer. The grain falls upon a double weigher d. This is shown in detail in Fig. 4, and consists of two drums R R_1 which are pivoted on a horizontal axle T, and can thus be brought, the one or the other, in alignment with the opening of the hopper V. As soon as one of these drums is filled, it tips over and discharges, while the other drum steps into charging position.

The following table shows the power consumed by the elevator here described:-

Quantity of Grain Handled per Hour. (Tons.)	Horse-power Consumed.
20	42
40	75
75	135
100	175
150	250

One of our half-tone engravings shows an important installation constructed on this principle at Brake on the Weser (Germany). This comprises four conveyers with

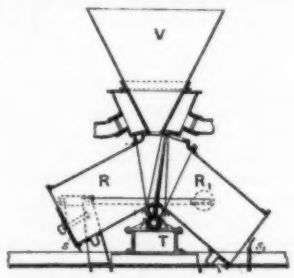


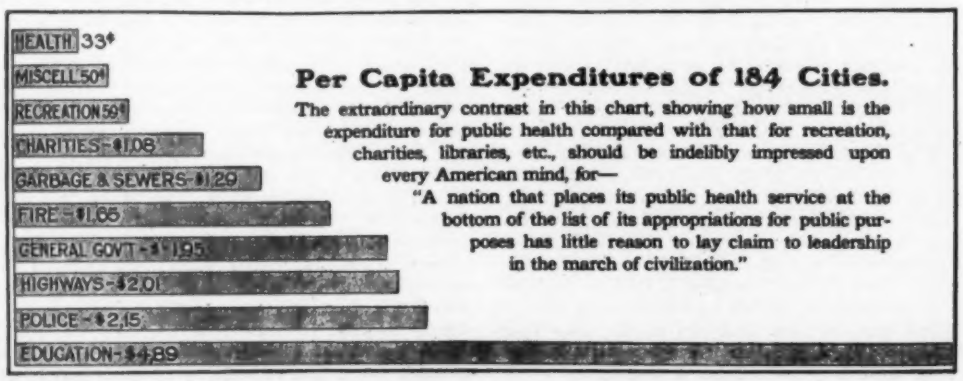
Fig. 4.—Section through automatic weigher of pneumatic grain conveyer.

a total capacity of 800,000 pounds per hour. The first three of these are placed on floats, the fourth is attached to a grain storehouse. The power required for the elevator and accessories is derived from five rotary motors having an aggregate capacity of 1,000 horse-power.

* Translated for the Scientific American Supplement from *La Nature*.

Expenditures of One Hundred and Eighty-four American Cities

We reproduce from *The Human Factor* a diagram which eloquently speaks for itself. Of course, there is one comment that suggests itself: It is not fair to compare directly the money expended on "Health" with that spent, for example, on "Highways"; for the simple reason that our requirements in dollars and cents may be much greater as regards the latter item. Indeed, in an ideal case, the expenditures for "health" would be nothing. This is evident enough if we consider an individual family. It does not follow that because one family spends ten times as much as another on doctor's bills, therefore it is ten times more healthy. But even making every allowance for this imperfection in the standard of measurement underlying the accompanying diagram, we have here ample food for serious thought and plans of reform.


HEALTH 33¢
MISCELL 50¢
RECREATION 56¢
CHARITIES-$1.08
GARBAGE & SEWERS-$1.29
FIRE-$1.65
GENERAL GOVT-$1.95
HIGHWAYS-$2.01
POLICE-$2.15
EDUCATION-$4.89
Per Capita Expenditures of 184 Cities.
The extraordinary contrast in this chart, showing how small is the expenditure for public health compared with that for recreation, charities, libraries, etc., should be indelibly impressed upon every American mind, for—
"A nation that places its public health service at the bottom of the list of its appropriations for public purposes has little reason to lay claim to leadership in the march of civilization."

Giant Drilling Machine for Turbine Work*

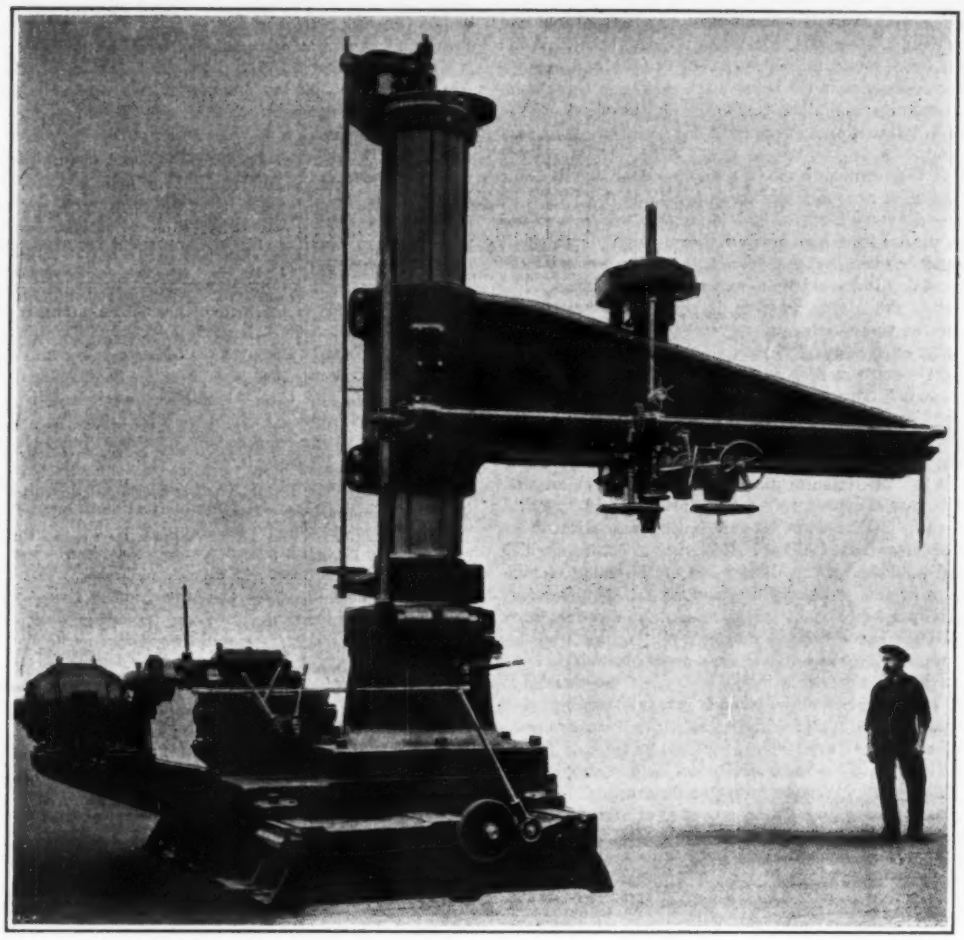
We illustrate herewith a large radial drilling machine designed primarily for work on turbine-casings. This type of machine, of which the spindle has a radius of 10 feet, is constructed by a Halifax firm. It is of very rigid design and has several good features. While its maximum radius is 10 feet, the spindle can be brought in to a minimum radius of 2 feet 10 inches, and the radial arm is capable of vertical adjustment, so that work varying between 3 feet 9 inches and 8 feet in height can be taken under it.

The spindle, which is 3¼ inches in diameter over the driving part, has a feed of 20 inches, with 16 speeds varying from 17 to 300 revolutions per minute, the number of feeds being 6. The spindle can be quickly adjusted by means of a rack-and-pinion motion. Its thrust is taken up by ball-bearings. A clutch is provided for the spindle, so that it can be started, stopped, or reversed without changing the main drive, and in tapping the reverse can be accomplished at a quicker speed than the feed. Variable self-acting feed is provided, capable of being changed while the machine is in motion, by simple lever movements. There is also a fine hand adjustment to the feed, while a friction feed arrangement, capable of being engaged or disengaged instantaneously, enables the spind'e to be adjusted up or down without knocking off the regular feed motion. An automatic trip to the feed is provided for drilling to any required depth.

The spindle-slide can be locked in position on the arm which carries it. The latter is of box section, the spindle working between the two sides, thus ensuring central thrust and absence of twist. The arm has vertical power adjustment of 4 feet 3 inches. It is provided with a fine radial motion, allowing accurate power adjustment. This can be quickly thrown out of gear when the arm has to be swung through a considerable arc. The arm is attached to the pillar by means of a long sleeve, which has ball and roller bearings, so that it can be swung by hand with very little effort. It can a'so be rigidly locked in any desired position. The gears of the gear-box are all enclosed in an oil-bath casing.

The machine has a short traverse bed, upon which it can be moved so as to cover a larger range of work than would be possible if it were fixed. Turbine-casings, for drilling which these machines are specially intended, are placed in a pit in front of the bed. The machine is provided with a 17-brake-horse-power constant-speed motor, and is suitable for drilling holes up to 4 inches in diameter or for tapping work. Its weight is approximately 22½ tons.

* Reproduced from *Engineering*.



This drill has a maximum radius of 10 feet and a minimum radius of 2 feet 10 inches.

The Influence of Antivivisection on Character—I*

Misrepresentations, Falsehood and Passion the Characteristics of Antivivisection Propaganda

By W. W. Keen, M.D., Philadelphia

"In this controversy (vivisection) there should be no bitterness. . . . Do not let us attempt to browbeat or call names. . . . Vivisection tends to weaken character. . . . Nothing which hurts the character can be right."—Rev. Dr. Floyd W. Tomkins, President of the American Antivivisection Society, in the *Ladies' Home Journal*, March, 1910.

I accept the test proposed by Dr. Tomkins, and quoted in the above motto, "Nothing which hurts the character can be right." Let us, therefore, study what is the effect of antivivisection on the character of its advocates.

1. VIOLENT PASSIONS AROUSED BY ANTIVIVISECTION AGITATION.

The most violent and vindictive passions have been aroused and fostered, especially among women—the very flower of our modern civilization. Let us see whether they have shown "bitterness" or "called names." I have rejected much oral testimony I could use and have drawn my evidence from only a very small portion of the literature at my disposal.

Herewith I reproduce (Fig. 1) the photograph of a remarkable letter which contains an asserted prayer to the Deity calling down curses by "a dozen women" on my long-since sainted mother. It needs no comment from me save that the "horror" mentioned in this letter was excited by an article which I published in the *Ladies' Home Journal* for April, 1910, in which I recited a few of the *benefits to humanity* which had resulted from vivisection. The only clue even to the place from which the letter comes is the postmark.

* An address read before the Surgical Section of the Suffolk District Medical Society, Boston, March 20th, 1912. Reprinted by the kind permission of the editor and the publisher from the *Boston Medical and Surgical Journal*, May 2nd and 9th, 1912.

Arch-Fiend:

I read with horror you article in the Ladies' Home Journal on vivsection.

I hope your mother if she is living will die in the most terrible torture, and if she is dead that her soul will never know rest for having given life to such a vile monster as you is the nightly prayer of

a dozen women who

indited this

Fig. 1.—Photograph of a letter received by myself in August, 1910. Both envelope and letter were typewritten, with no place, no date and no signature; only the postmark showed that the letter came from Los Angeles.

Let me quote another earlier anonymous letter I have before me. This is from Philadelphia. Instead of the usual address "Dear Sir," it begins, "You Fiend." I had not then been promoted to "Arch-fiend" in Satan's hierarchy. The writer exclaims, "Oh, that you all could be put through the same torture that you inflict on these helpless ones." As I am not a vivisectionist this ardent wish fails to terrify. I am an advocate of vivisection because I *know* how greatly it has helped me during all my professional life in saving life and lessening suffering.[1]

If two letters will not convince, here is a third. This, from Baltimore, also the result of the same article, was from a writer who had the courage to sign her name and address:

"You would appear even the more fiendish on account of your superior intelligence. . . . The future of a vivisectionist is a veritable hell. You, I understand, are

[1] In the *American Journal of the Medical Sciences* for July, 1865, p. 67, Dr. S. Weir Mitchell, the late Dr. Morehouse and I published a paper on the "Antagonism of Atropia and Morphia," based on observations and experiments in the Army Hospital for Injuries and Diseases of the Nervous System. The reason which caused us to make this investigation was that we desired to find better means for "soothing the pain of those terrible cases of neuralgia" following gunshot-wounds of large nerves. These are accurately described in the paper as causing "anguish" and "agony"—no word could be too strong. Accordingly, in our efforts we tried a number of common and some uncommon drugs, and finally found that morphine (the active principle of opium) was the best remedy and yet had many disadvantages. Ultimately, we found that by combining with it a certain amount of atropin (the active principle of belladonna) we obtained the best results. The facts discovered in our investigations have long since become merged in the common knowledge of the profession, and standard tablets with different proportions of the two drugs are manufactured and used all over the world. Most of our patients operated on (entirely by hypodermic injections) were sorely in need of relief. A few were convalescents. In all cases we avoided telling them what drug was being used, for everyone knows how imagination, fear or other emotion would alter the rate of the pulse or of the breathing. Not one man was injured in the least. Not one ever complained. Many thousands of human beings have been greatly benefitted, and many lives have been saved through the knowledge thus obtained.

I have expressly mentioned these facts in some detail because we have been attacked in their pamphlets by the antivivisectionists for these experiments, which are described as "human vivisection."

a man advanced in years [the calendar, alas! seems to justify this shocking statement] soon to go before the bar of justice. Can you meet your God with the terrible cries ringing in your ears of these creatures, our helpless brothers, made by His hand, that you have drawn and quartered? How they must haunt you. . . . When your time comes to die, every cry of pain and anguish that you have been the cause of producing in these helpless creatures will follow you to the depths of hell." Yet I have "drawn and quartered" not even so much as a mouse.

But this same lady tells me that she had survived one of the most serious abdominal operations that could be done—a hysterectomy. This operation was so perilous that until Lister had devised the antiseptic method it was never even thought *possible*, and its success at the present day is due chiefly to experiment on animals. The writer of the letter, therefore, is herself a witness to the benefit of vivisection.

Later on she says, "If they would only use vivisectors for their experiments, it would soon be considered unnecessary." Her gentlest wish, therefore, is for human vivisection, and doubtless "without anesthetics." *Per contra*, in the newspapers of May 6th, 1911, a dispatch states that seventeen medical students had offered themselves for experimental inoculation with cancer, an offer which was, of course, refused, as animals can be used.

If three instances are not sufficient, here is a fourth—a signed letter from Chicago. Referring to one case which I had published as an illustration of the value of vivisection in saving human life, she says, "My sympathy for the parents of that young man . . . would have been deep, but not so keen as for a mother dog who saw her puppy tortured to death on a dissecting table. . . . Even if you did save a man's life, *was it worth while?*" (Italics in the letter!) This lady wrongly assumes that the puppy was "tortured to death," i. e., without anesthetics. This, I am glad to say, is not true, as I shall show later on. To her question, "Was it worth while?" I can only say, "Ask his father and mother."

And this is the ennobling influence of antivivisection!

Antivivisection writers nearly always state, assume or imply that all experiments are "tortures," i. e., that anesthetics are not used. This is wholly erroneous.

In Great Britain, where all experiments are returned to the government, the following table for 1906 (the latest I happen to have) will show how utterly indefensible is such an assumption. It is a fair presumption that about the same average exists in the United States.

	Per cent.
Inoculations, etc., not involving any operation	93.96
Animals killed under anesthetics	3.44
Animals allowed to recover from anesthetic but nothing likely to cause pain and no further operation allowed without anesthetic	2.60
	100.00

In other words, only *26 animals out of 1,000* could by any possibility have suffered *any* pain, and very few of these any serious pain. Is this the torture and agony so constantly harped on?[2]

Many of the instances cited in antivivisection literature are taken from researches—such as Magendie's—which were made before anesthetics were discovered, over 65 years ago.

The rest in which real cruelty was inflicted, and which, if done now, would be condemned by all modern research workers as freely as by the antivivisectionists themselves, were done almost wholly on the Continent, and often by persons who are now dead. In discussing vivisection to-day, these should be excluded, or their dates and countries indicated, for the public, ignorant of medical history, is misled into supposing that these persons are living and practising these methods to-day and in America.

In one of the anonymous replies to my paper on the "Misstatements of Antivivisectionists," I am represented as the apologist and advocate of experiments of which twice over at the Senate Committee hearing and again in my letter to Mr. Brown I had expressed my utter disapproval. I am always willing to face a truthful charge, but it is a hopeless task to meet untruthful charges, especially when the author is ashamed of his own name. "Hell at Close Range" is the title given by Miss Ellen Snow to a leaflet dealing with the work of the Rockefeller Institute. One would scarcely expect such a fierce heat from so frosty a name.

At this institute, by experiments on 25 monkeys and 100 guinea-pigs, most of which animals recovered, has been discovered a serum that has brought the former death-rate of cerebrospinal meningitis of 75 or 90 per cent down to 20 per cent and less. Is it because of this beneficent work that it is called "Hell"?

At this institute has been discovered a means of transfusion of blood that has already saved scores of human lives. Is this the reason for calling it "Hell"?

At this institute, a method of criss-crossing arteries and veins, which almost always run alongside of each other, has been discovered by which impending gangrene has been prevented. Does this make it à "Hell"?

At this institute, the cause and the cure of infantile paralysis are being sought. Are such investigations carried on in "Hell"?

Miss Snow, in this same leaflet, expresses in italics her horror at the idea of the proposition of the institute "to build a hospital where the experiments may be continued on human beings." It may be of interest to her and also to others to know that this hospital was opened in October, 1910, and that the public, undeterred by her horror, have thronged to it in such numbers that there have not been beds enough for the several hundreds of disappointed applicants.

An editorial in the *Journal of Zoophily*[3] records a gift to this Rockefeller Institute, "an institution in New York where vivisection should be practised with the idea of achieving as great an advance as possible in the war of science against human suffering," and adds, "but the gift only fanned into fury the opposition of the women to experiments on living animals, *no matter how great the anticipated benefit.*" Could cruel passion be better expressed?

Can a cause which so seriously injures the character of its advocates that they indulge in this prolific vocabulary of vituperation by any possibility have an uplifting influence? It eminently fulfils the proposed test—it "hurts the character and, therefore, cannot be right."

Are those who give loose rein to such passion fitted to form a sound and sane judgment on the subject about which they write? This is especially true when the matter is one so technical as anatomic, physiologic, chemical, pathologic and surgical investigations as to which they cannot be expected to know and, in fact, do not know anything. Even relatively few medical men are fitted by temperament and training to act as censors of such researches, much less those ignorant of medicine.

I believe that much of the passion shown in the above quotations is the result of ignorance. Most of the attacks on vivisection, as I have said, assume or even state categorically that anesthetics are not used. Saving in the very rare cases in which the use of anesthetics would entirely frustrate the experiment, anesthetics are always used. This is done not only for reasons of humanity, but also because the struggles of a suffering animal would make delicate and difficult operations absolutely impossible, to say nothing of the danger of injury to the operator.

The always-quoted opinion of Prof. Bigelow was founded on what he had seen at the Veterinary School at Alfort, France, in the preanesthetic days. Many absolutely false statements are made that anesthetics were not used in certain specified experiments, whereas the experimenters have expressly stated that anesthetics *were* used. Of such misstatements by antivivisection authors I shall give some startling instances later. It is no wonder that the public has been thus misled. "Cutting up men and women alive" is an accurate description of every surgical operation, but we all know that while in comparatively few reports of surgical operations it is expressly stated that an anesthetic was used, such use "goes without saying."

One of the most frequent antivivisection statements is that "incomplete" or "slight" or "light" anesthesia means that the animal is fully able to feel pain and that when the eye resents a touch or there is muscular movement following any act which would be painful when one is not anesthetized, pain is actually being inflicted. Mr. Coleridge says (Question 10,387 in his testimony before the Second Royal Commission on Vivisection), "What does 'anesthetized' mean? It means 'without feeling.' You cannot be slightly without feeling. You either feel pain or you do not."

In modern laboratory researches, ether or other anesthetics are almost always given. Extremely few exceptions occur, and then only with the consent of the director in each specific case. The actual conditions at the present day are well shown by the rules in force in practically all American laboratories of research. These rules have been in operation for over 30 years in one case and for more than 10 years in others. In most laboratories in which students work, and where they are absolutely under the control of the director, the only animal used is the frog, and by "pithing" or decapitating it, it is made wholly insensible to pain.

The idea that students privately "torture" animals, often, it is stated, out of mere curiosity, is absolutely false. I have been intimately associated with students ever since 1860, first as a student and since 1866 as a teacher. I state, therefore, what I am in a position to know. Moreover, private experimental research takes time which our overworked students do not have, and money which they cannot afford. It means the rent of a laboratory, the purchase of very expensive and delicate instruments, the rent of an animal room, the cost of the animals, and of their food and care, a man to look after them—for all modern surgical work on animals must be done with the same strict antiseptic care as on man, or the experiment will surely fail and discredit the author—a total expense amounting to a very large sum.

I quote in full the rules which, as I have said, are in force in practically all American laboratories of research:

RULES REGARDING ANIMALS.

1. Vagrant dogs and cats brought to this laboratory and purchased here shall be held at least as long as at the city pound, and shall be returned to their owners if claimed and identified.

2. Animals in the laboratory shall receive every consideration for their bodily comfort; they shall be kindly treated, properly fed, and their surroundings kept in the best possible sanitary condition.

3. No operations on animals shall be made except with the sanction of the director of the laboratory, who holds himself responsible for the importance of the problems studied and for the propriety of the procedures used in the solution of these problems.

4. In any operation likely to cause greater discomfort than that attending anesthetization, the animal shall first be rendered incapable of perceiving pain and shall be maintained in that condition until the operation is ended.

Exceptions to this rule will be made by the director alone, and then only when anesthesia would defeat the object of the experiment. In such cases, an anesthetic shall be used so far as possible, and may be discontinued only so long as is absolutely essential for the necessary observations.

5. At the conclusion of the experiment, the animal shall be killed painlessly. Exceptions to this rule will be made only when continuance of the animal's life is necessary to determine the result of the experiment. In that case, the same aseptic precautions shall be observed during the operation, and so far as possible the same care shall be taken to minimize discomforts during the convalescence as in a hospital for human beings.

(Signed) .....

Director of the Laboratory.

I may add that at the Rockefeller Institute, regular trained nurses are employed and are on duty not only during the day, but at night when necessary.

Self-confessed total ignorance of a subject on which one gives extensive evidence is not often known, but Dr. Herbert Snow, of London, an authority among the antivivisectionists, is a case in point. Dr. Snow's evidence before the Royal Commission on Vivisection (1906) covers 10 pages quarto and he answers 326 questions. In 1911, Dr. Snow visited America. In a letter to the Philadelphia *Ledger*,[4] he makes the almost incredible statement that he gave all this evidence "in utter ignorance of the vivisection question."

Moreover, when asked by the Commission (Question 2242), "Do you find any fault with the present gentlemen who are licensed under the act?" he replied, "I do not" and again (Questions 2227 and 2228) he admits that both painful and painless experiments may sometimes be necessary.

In other cases, ignorance of physiology and anatomy is shown which would only create a smile did it not gravely mislead the reader. I shall give only a single illustration here. Others will be found elsewhere in this paper.

"The Nine Circles," with its sulphurous subtitle, "Hell of the Innocent," is an English book originally issued by the late Miss Frances Power Cobbe, in 1892. This edition had to be withdrawn on account of its false statements, especially as to the non-use of ether.[5] A second and revised edition was issued in 1893. This was "carefully revised and enlarged by a subcommittee especially appointed for the purpose," as the preface states.

On page 15 of the revised edition, it is correctly stated that Prof. Henry P. Bowditch of the Harvard Medical School, in some experiments on the circulation, etherized a cat and that "then its sciatic nerve was divided," etc. The sciatic nerve is the largest nerve in the body of man and animals and passes down the back of the leg. After division of the nerve the portion going down the leg *below* the place where the nerve was divided was stimulated by an electrical current. As this part of the nerve was wholly cut off from the spinal cord and brain, *by no possibility could any pain be felt.* Yet a Boston lawyer, in a leaflet published by the New England Antivivisection Society, comments on a similar experiment as follows: "It will be readily seen even by the casual reader that it involves an amount of agony beyond which science is

[2] Since this address was delivered, the report of the British Royal Commission on Vivisection, on which the antivivisectionists were represented, has appeared. One of their unanimous conclusions (page 20) is as follows:

"We desire to state that the harrowing descriptions and illustrations of operations inflicted on animals, which are freely circulated by post, advertisement or otherwise, are in many cases calculated to mislead the public, so far as they suggest that the animals in question were not under an anesthetic. To represent that animals subjected to experiments in this country are wantonly tortured would, in our opinion, be absolutely false."

This clear statement should end this calumny.

[3] *Jour. Zoophily*, January, 1909, p. 2.

[4] *Philadelphia Ledger*, March 6th, 1911.

[5] We shall have occasion to refer to this again later.

unable to go." Just how the "casual reader" would be so well informed as to physiology when a lawyer and two doctors—not casual but intelligent and careful readers—got things totally wrong, is not stated. Dr. Bowditch published a correction[6] of this misstatement in 1896. In spite of this, the New England Antivivisection Society in 1909, *thirteen years* after this public correction, was still distributing this lawyer's statement.

But in "The Nine Circles" (second edition, carefully revised by Dr. Berdoe and the committee) these experiments are referred to as "experiments on *the spinal cord*"! (Italics mine.) Yet Bowditch did no operation on the spinal cord. Miss Cobbe, not being an anatomist, might be pardoned for confusing the thigh and the spine of the cat, but surely Dr. Berdoe ought to have seen to it that "sciatic nerve" and "spinal cord" were not used as interchangeable terms.

Many years ago, after amputating a leg near the hip, I tried to see how long electric stimulation of the sciatic nerve would cause the muscles of the amputated leg to contract. After four hours, during all of which time the muscles continued to react, I had to stop as I could give no more time to the experiment. According to the canons of antivivisection as voiced above, I should have continued to etherize the patient whose leg had been amputated, for he, just as much as Bowditch's cat, could feel "agony beyond which science is unable to go."

Let me give one other surprising statement. Dr. Hadwen[7] vaunts the use of salt solution instead of the direct transfusion of blood, and rightly says that he has "seen the most marvelous effects follow the injection of an ordinary saline solution into the venous system in cases of loss of blood." But he seems to be ignorant of the fact that this very saline transfusion was begun and perfected by experiments on animals. I commend to him Schwarz's essay (Halle, 1881) with its 24 experiments on rabbits and dogs, and Eberius's essay (Halle, 1883) with its 10 experiments on rabbits and the record of 11 cases in which Schwarz's method had already been used in man. These essays were practically the beginning of our knowledge of the advantages of the use of salt solution over the old dangerous methods of transfusion of blood.

The antivivisectionists deny the truths of bacteriology. Yet we practical physicians, surgeons and obstetricians *know* by daily experience that Pasteur's and Lister's researches are the basis of most of our modern progress. Are Hadwen, Harrigan, Snow and their colleagues right and have all medical colleges all over the world in establishing chairs of bacteriology and all medical men in believing bacteriologic diagnosis of such importance and in basing on the germ theory their antiseptic treatment which has so revolutionized modern surgery, been wholly wrong? The germ theory is as well established as the doctrine of the circulation of the blood.

II. FOSTERING A SPIRIT OF CRUELTY TO HUMAN BEINGS.

My second reason for believing that antivivisection injures character is that, by putting a greater value on the well-being and the lives of monkeys, guinea-pigs, rabbits, dogs, cats, mice and frogs than on the lives of human beings, it fosters a spirit of cruelty to human beings.

Is it not a cruel passion which will lead men and women to write such letters and to print such epithets as I have quoted? Is it a right thing to misstate the facts of operations, and after the falsity of the charge has been proved, still continue for years to hold up men with human feelings and sensitive to abuse before the community as vile monsters of cruelty? Nay, more than this, is it not an extraordinar thing that those who so vehemently denounce human vivisection are even among its advocates?

When I was professor of surgery in the Woman's Medical College of Pennsylvania, I took as the topic of my address at one of the commencements, "Our Recent Debts to Vivisection." Mrs. Caroline Earle White published "An Answer to Dr. Keen's Address Entitled, 'Our Recent Debts to Vivisection.'" At the bottom of page 4, I find the following: "I take issue with Dr. Keen in the second place where he says: 'These experiments cannot, nay, must not, be tested first upon man.' I assert, on the contrary, that *in the majority of cases they must be tested first upon man* [italics my own] or not tested at all, because no important deductions can ever be drawn with any degree of certainty from experiments upon animals, since in some inexplicable way their construction is so different from that of man."

The statements in the latter portion of the concluding sentence will much amuse anatomists, physiologists and biologists, or, in fact, anyone who really knows anything about science. With minor modifications, man and the lower animals are alike in almost all particulars, both in structure and function, in health and disease.

The extraordinary fact is that Mrs. White asserts that experiments must be tested first on men or not tested at all. That is to say, we must either experiment on human beings or else continue in exactly the same old rut as before and never make any progress, for every departure from prior practice, however slight, is an "experiment."

If this basic doctrine of antivivisection had held good for the last 50 years Lister would not have been able, after carefully testing his antiseptic method on animals and having found it successful, then, and not before then, to try it on man.[8] By this means he became, as the *British Medical Journal* has just called him, "the maker of modern surgery."

On page 10 of Mrs. White's "Answer" is found the following flat-footed advocacy of human vivisection: "Dr. Keen mentions that in India alone 20,000 human beings die annually from snake-bites and as yet no antidote has been discovered. How can we search intelligently for an antidote, he says, until we know accurately the effects of the poison? I should reply that in order to find out the effects of the poison and to search also for an antidote, the best plan would be for the experimenters to go to India where they could find as large a field for investigation as they require in the poor victims themselves. *Here is an opportunity such as is not often offered for experimenting upon human beings,*[9] since as they would invariably die from the snake-bites, *there can be no objection to trying upon them every variety of antidote that can be discovered.* Nothing seems to me less defensible than these experiments on the poison of snake bites upon animals since it is the one case in which they could be observed with so much *satisfaction and certainty upon man*[8]" (Italics my own.)

Such a proposal is as absurd as it is cruel. Even if the experimenter could afford sufficient time and money to go to India for months or rather for years, how could he arrange to be present when such unexpected accidents occurred? How could he have at hand in the jungle the ether, chemicals, assistants, tables, tents, food and drink, and the necessary yet intricate and delicate instruments? And even if he had all of these, how could he work with the calmness and the orderly deliberation of the laboratory when a fellow human being's life was ebbing away and every minute counted in such a swift poison? The proposal is cruel and revolting and would never be accepted by any investigator.

But Mrs. White is not the only one who is guilty of making such a proposal. Many antivivisection leaflets and pamphlets express the wish that the vivisectors should be vivisected. In a pamphlet[10] freely distributed in the United States I find the following in a letter from a man at that time a Senator of the United States: "It would be much better to dissect men alive occasionally for the general welfare because the attendant phenomena and demonstration of the victims being of our own particular form of animal would be far more valuable than the result of our observation upon the physical structure illustrated in the agonies unto death of the helpless creatures around us." The English is as distressing as the proposal is astounding.

Let me give one more illustration of the effect of antivivisection in encouraging cruelty.

To-day the plague, cholera and yellow fever no longer terrify Europe or America. What is the reason for this? Primarily and chiefly the discovery of the germs of cholera and of the plague by bacteriologic methods, which in turn are very largely the result of experiment on animals, and of the means of the transmission of yellow fever, though as yet not of its cause. In the latter case, experiments on animals were out of the question because it is impossible to transmit yellow fever to animals. They are not susceptible to the poison. So a number of noble medical men and others volunteered to have experiments tried on them. The very first experiments were tried on medical men. These men slept in a stifling atmosphere for twenty nights in the beds in which yellow fever patients had died, and in their very clothes, clothes soiled with their black vomit, urine and feces; tried to inoculate themselves by putting some of the black vomit into their eyes, or by hypodermic injections, etc., but all in vain. By none of these methods were they able to inoculate themselves with the fever. One step more was requisite—to learn whether a well man bitten by an infected mosquito, but having been exposed to no other possible source of infection, would contract the disease. Dr. Carroll of the Army was the first to offer himself, and nearly lost his life. Others followed. Several lost their lives, among them Dr. Lazear, at the beginning of a most promising career. His tablet in the Johns Hopkins Hospital, in the fine words written by President Eliot records that "with more than the courage and the devotion of the soldier, he risked and lost his life to show how a fearful pestilence is communicated and how its ravages may be prevented."

Contrast with this a cruel letter[11] written by a woman: "Science is based on such firm foundation, indeed, that it can at a moment's notice be tumbled down and become a wrecked mass by a mosquito! Not only this, but these life-long vivisectors could not even prolong their own lives. Undone by a mosquito! *I shall always have unbounded admiration for that clever insect.*" (Italics mine.)

This self-sacrifice for humanity has made us masters the world over of yellow fever, has made possible the Panama Canal, has saved many thousands of human lives and millions of dollars in our own Southern States alone, and yet a woman can feel "unbounded admiration for the clever insect" which slew these heroes and had devastated cities and countries for centuries! Does not such antivivisection zeal "hurt character"?

(*To be concluded.*)

[6] Bowditch, Henry P.: Advancement of Medicine by Research, p. 43.

[7] Hadwen: *Jour. Zoophily*, January, 1910.

[8] Keen, W. W.: Modern Antiseptic Surgery and the Rôle of Experiment in Its Discovery and Development, *Journal American Medical Association*, April 2nd, 1910, p. 1104.

[9] In her answer to this address (*Boston Medical and Surgical Journal*, July 25th, 1912, p. 143), Mrs. White, after ample time for reflection, defends her proposal for "experimenting on human beings," saying that "it does not seem to me that this is a cruel suggestion, as my only object in it was to benefit the poor natives who die by the thousand every year." Such a defense places her clearly and definitely among the advocates of vivisection, whose "only object" is to prevent death "by thousands every year." This object, moreover, has already been obtained in a score of diseases and will be obtained hereafter in many others, not, however, by "experimenting on human beings," as she advocates, but on dogs, cats, rabbits, guinea-pigs, mice, frogs, etc.

[10] Cobbe, Frances Power, and Bryan, Benjamin: Vivisection in America, p. 15.

[11] *New York Herald*, August 2nd, 1909.

An Arithmetical Curiosity

A. C. D. Crommelin in the *Journal of the British Astronomical Association* writes:

In an old logarithm book, I came across the problem: "What is the largest number that we can express by three digits?"

The answer is, of course, $9^{(9^9)}$; it is a number of inconceivable and appalling magnitude, compared with which the huge numbers that we meet with in sidereal astronomy fade into absolute insignificance: 9^9 is 729^8, which is easily found to be 387,420,489. We have to multiply log 9 by this figure in order to obtain log 9^{9^9}. Seven-figure logs are useless; we must employ at least 10-figure ones to get any significant figures in the answer. I had recourse to the 61-figure logs of certain numbers computed by Abraham Sharp, and given in Hutton's tables. From these, I find that log 9^{9^9} = 369,693,099.63157, 03587, etc. Hence the number contains 369,693,100 figures. I have found the first 28 of them to be 428, 124, 773, 175, 747, 048, 036, 987, 115, 9. We also know that the two final figures of the number are 89, since odd powers of 9 end successively with 09, 29, 49, 69, 89, 09, etc. A knowledge of 30 figures out of 300 million may seem trifling, but in reality the error involved in taking all the remaining figures as zeros is only one part in a thousand quadrillions. If the number were printed with 16 figures to an inch (about the tightest packing for decent legibility), it would extend over 364.7 miles, or from London to Perth. If printed in a series of large volumes, we might get 14,000 figures to a page, and with 800 pages to the volume it would fill 33 volumes. There are more than twice as many digits in the number as there are letters in the whole of the Encyclopædia Britannica.

To find the largest number suggested by sidereal astronomy, I took the following: Both Very and See have expressed the opinion that certain visible objects may be at a distance of a million light years. I imagine a solid sphere of platinum of this radius, and find how many electrons it contains. From Duncan's "The New Knowledge," page 65, I find that the log of the number of electrons in a cubic centimeter of water is 16.469. Taking the density of platinum as 21.5, the log of the number of electrons in a cubic inch of it is 19.016, and the log of the volume of the huge sphere in cubic inches is 71.3362. Whence the log of the number of electrons it contains is 90.352, and the corresponding number is 225 followed by 88 zeros. At 16 figures to the inch, this would take 5¾ inches, less than the width of two columns of the Scientific American Supplement.

It is interesting to note that the number of electrons in the body of each human being would suffice to change about 21 of the 91 figures.

To find the radius of a sphere of platinum that would contain 9^{9^9} electrons, we must multiply our million-light-year radius by a number whose log is 123,231,003.093, i. e., the multiplier is 1239 followed by 123,231,000 zeros. In fact, that gigantic sphere would exceed the million-light-year sphere in a far higher ratio than that exceeds the size of one electron. Hence, we may take it as morally certain that we can write with three digits a number vastly exceeding the number of electrons in the whole of creation, which is a somewhat startling fact. Indeed even the number 4^{4^4} (which is 13,407,813 followed by 147 other figures) probably exceeds the number of electrons in creation. At least, it equals the number of electrons in a solid platinum sphere that exceeds the million-light-year sphere in the same proportion that that exceeds a sphere 206 inches.

A correspondent some time ago wrote to us on this subject, and pointed out that a vastly greater number is $\frac{1}{2}9!^{(9!^{9!})}{}_{!}^{!}$, where n! is written for $1 \times 2 \times 3 \times \ldots \times n$, in accordance with a common notation.

Can any of our readers suggest some method of estimating at least the order of magnitude of this number?

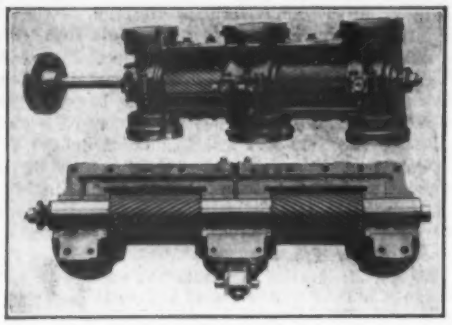


Fig. 1.—View of one of the pinions and floating frames, showing also lubricating grooves.

Propelling Machinery for the Collier "Neptune"*

The Reduction Gear and Dynamometer

Fig. 2.—One of the pinions and half its floating frame in position.

[*In our issue of May 30th we gave an illustrated description of the turbines on the U. S. S. "Neptune." In the present number we bring an account of the reduction gear and dynamometer of this ship.*—EDITOR.]

IT is well known that when a shaft is transmitting power, the torque exerted creates a definite twist or torsional deflection which is directly proportional to the load transmitted and inversely proportional to the cube of the shaft diameter. This deflection of shafts, when transmitting large power, is the basis of practically all of the torsional dynamometers used to determine the amount of power transmitted to the propellers in marine turbine installations. Various methods are used to indicate the torsional deflection from which the torque transmitted is calculated.

The pinion of a reduction gear may be considered as an extension of the turbine shaft, and as such it will have a certain deflection proportional to the power transmitted from the turbine to the propeller. Consequently, the driving end of the pinion will be advanced with respect to the end most remote from the point of application of the driving force. Since it is impossible for slip to occur between the pinion and the driven gear, the end of the pinion nearest the point of application of the driving force will tend to climb or advance along the driven gear slightly ahead of the end of the pinion farthest removed from the point of application of the driving force.

If we were to assume then that the bearings in which the pinion is supported were fitted very loosely with a considerable clearance around the pinion journals, the pinion would climb or advance along the face of the driven gear at the driving end by an amount equal to the torsional displacement of the teeth between the near and far end of the pinion, thus causing the axis of the pinion to incline itself at a slight angle to a plane passing through the center of the axes of the gear and the pinion when the latter is not subjected to the twisting force.

Thus it is seen that the *natural* position taken by a pinion transmitting power is not with its axis parallel with the axis of the driven gear, but at an inclination, the angle of inclination being proportional to the twisting moment applied to the pinion which, in marine reduction gears, is proportional to the square of the revolutions per minute. Thus it will be seen that no fixed adjustment of any nature whatsoever can compensate for the torsional deflection at all powers and speeds, and any permanent adjustment for some fixed load would cause unequal pressures at all other loads.

This is the basic principle which is made use of in the Westinghouse floating frame, the floating frame being merely a means of permitting the pinion to assume its *natural* position without imposing unnatural stresses on the bearings and teeth. It will be readily seen that if the axis of the pinion naturally tends to incline itself to the axis of parallelism with the axis of the driven gear, any arrangement which prevents, or tends to prevent, this natural adjustment, must necessarily not only subject the teeth of the gear to unnatural and unnecessary stresses of considerable magnitude, but must likewise subject the bearings to similar stresses.

It may be difficult without previous consideration to appreciate the effect of the torsional deflection, as this is comparatively slight, and consequently the natural tendency is to conclude that it is insignificant. However, if the pinion were assumed to be made of rubber or some equally elastic material, the necessity for permitting the pinion axis to assume its own position without restraint will become more evident. If we were to assume the pinion made of rubber, it will be readily seen that even with a comparatively small twisting force the twist in the pinion would become so great that the teeth at the end farthest from the point of application of the driving force would fail to mesh with the teeth of the driven gear unless the axis of the pinion were free to tip sufficiently to permit the teeth at the end farthest from the point of application of the driving force to lag behind sufficiently to come into mesh.

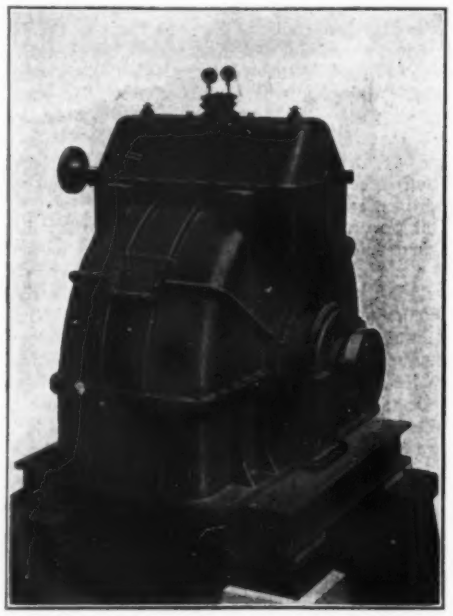


Fig. 3.—Gear and floating frame assembled and complete.

From the above it will be readily seen that unless the pinion is permitted to assume its own axis the pressure on the teeth will be greater at one end of the pinion than at the other, and consequently though the pressure per unit length of pinion face may be sufficiently low if uniformly distributed, it will be sufficiently high locally to disturb the oil film between the teeth and cause undue friction and rapid wear.

It will naturally occur to the reader that if the diameter of the pinion were made sufficiently large in proportion to the power transmitted, the torsional deflection and consequent error between the natural axis of the pinion and parallelism may be insignificant, and this is true within certain limits.

Increasing the diameter of the pinion has serious drawbacks in marine work, since an increase in the diameter of the pinion means a larger increase in the diameter of the driven wheel, the increase in the latter being greater than that of the pinion by the number of reductions of speed.

In order to obtain the full benefit of reduction gearing in marine installations, it is necessary to select the highest number of revolutions per minute permissible for the turbines, and as the propeller revolutions for maximum efficiency must be kept comparatively low, the reduction ratio desirable is generally very large; and consequently any increase in the size of the pinion in order to reduce the torsional deflection is accompanied by a considerable increase in the diameter of the large gears. Roughly, the weight of a reduction gear is proportional to the square of the diameter of the large gear plus the square of the diameter of the pinion, so it is evident that for a given ratio of reduction the weight of the reduction gears increases approximately as the square of the diameter of the pinion.

If it is attempted to avoid the abnormal increase in weight of the gear due to an increase in the diameter of the pinion for a given power transmitted by reducing the revolutions of the turbine, the result is that a considerable portion of the saving in weight in the gear will be counterbalanced by an increase in weight of the turbine, the weight of the turbines varying approximately inversely as the square of the revolutions per minute. Further, reducing the revolutions of the turbines below the maximum permissible for any given power, results in a decrease in economy, since a turbine reaches its maximum economy when the revolutions for the power developed are the maximum permissible. Likewise, the space occupied and the weight of the combined installation is considerably increased as the revolutions of the turbine are decreased, and a decrease in the revolutions of the turbine necessitates an increase in the diameter of the pinion, since the torque transmitted for a given horse-power is inversely proportional to the revolutions per minute.

By the use of the floating frame the size of the pinion required for any given horse-power is not limited by the diameter of the pinion, torsional deflection or strength of the teeth, but by the size of the bearings. Since it is not desirable to make the journals much larger than

* By courtesy of the Westinghouse Machine Company.

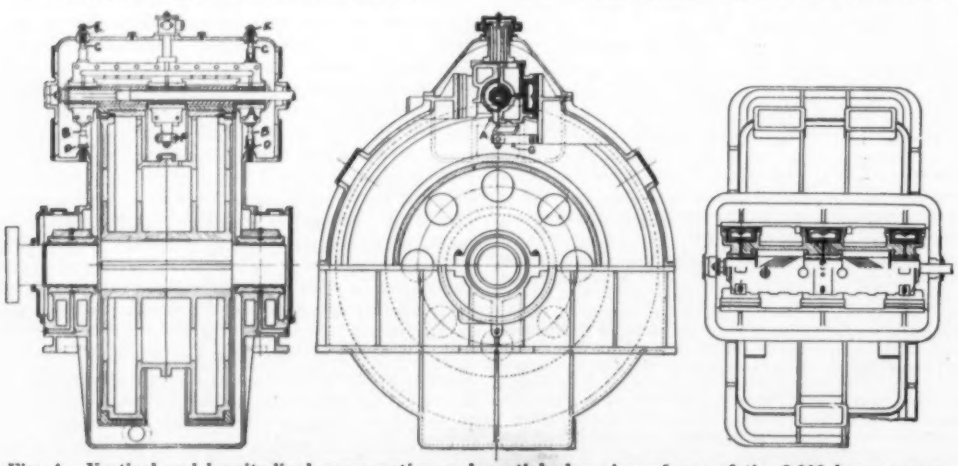


Fig. 4.—Vertical and longitudinal cross-section and partial plan view of one of the 3,000 horse-power reduction gears for the "Neptune."

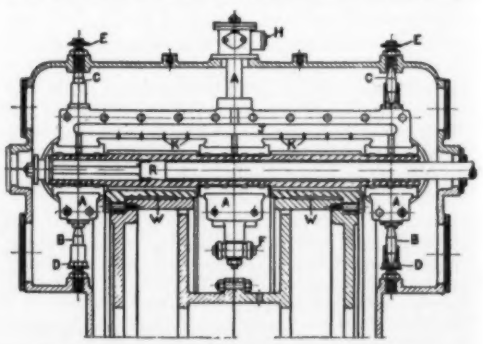


Fig. 5.—Details of floating frame and pinion.

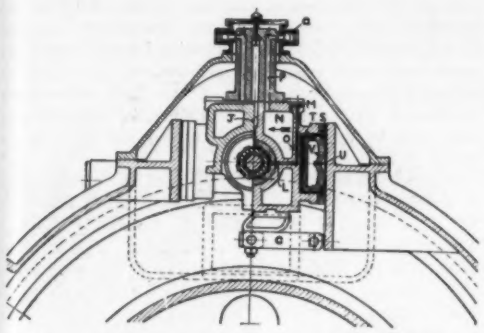


Fig. 6.—Details of floating frame and pinion.

the outside diameter of the pinion, and only certain maximum bearing pressures and surface speeds are permissible, the limit of capacity of the pinion is reached when the bearings are loaded to the maximum safe limit. In general, however, it is not necessary nor desirable to undercut the pinion teeth below the journals, and the pinion diameter is made just large enough so that the cutters clear the journals. Thus it is seen that the capacity of Westinghouse pinions of any given size is much greater than in any other system of gearing, and the size and weight of the gear are far less for a given ratio of reduction, or for any given limit of diameter of the large gear wheel, a large ratio of reduction may be used.

It will be readily understood from the preceding that particularly in marine installation where weight and space, as well as economy, are the most important factors, it is imperative to have some means of permitting the pinion to assume its natural position in order that a considerable torsional deflection is allowable, and consequently a small pinion diameter for any given power transmitted.

In many marine installations of reduction gears without means for permitting the pinion to assume its natural axis, the result of the necessity for an increase in pinion diameter and the consequent limited ratio of reduction is evident, as with two exceptions, in all the geared installations with solid pinion bearings, high-speed propellers running as fast or faster in some instances than would have been considered necessary in a direct connected turbine installation, have been employed, and in each instance the revolutions of the turbines were considerably lower than desirable for the power developed. Thus, though gears were added, no advantage could be taken of the increase of the efficiency of the propellers which would result from a lower propeller speed, and only part of the gain in economy was realized by increasing the revolutions of the turbines, though some saving in weight was obtained. In the Westinghouse gear the use of a pinion of minimum diameter and a high-speed turbine has the very marked advantage of permitting the use of very small turbines for the power developed, making all the parts small and easily handled in addition to the saving in weight and increased economy of the turbine and decrease of power required due to the use of slow-speed propellers of high efficiency.

In some instances in reciprocating engine vessels a lower speed propeller than desirable has been fitted to suit the engine speed, but with the reduction gear the propeller speed can always be made to meet the requirements of maximum propeller efficiency.

While in most cases the revolutions per minute of a reciprocating engine can be made high enough to give a maximum efficiency, it is, of course, well known that quite a considerable increase in propeller speed is permissible without decreasing the efficiency of the propeller.

There are a number of important advantages resulting from the use of the fastest running propeller which will give maximum efficiency, and these may all be secured by the use of reduction gears.

The higher propeller speed and consequent reduction in diameter permits deeper submersion of the screws and less liability to racing in heavy weather, thus allowing a high speed to be maintained. The deeper submersion also reduces the danger of the propeller striking floating objects which might cause the loss of blades.

With the reduction in the diameter not only is the original cost of the propeller materially reduced, but likewise the cost of renewals and repairs in case blades are lost, and, furthermore, the reduction in weight may be sufficient to warrant the use of bronze where otherwise cast iron propellers would be fitted.

The use of the highest propeller speed consistent with maximum efficiency also materially reduces the diameter of the tunnel and tail shafting and stern tube, and reduces the size of plummer and thrust blocks, resulting in a very substantial saving in weight. Also the tunnel shafting of small diameter is not subjected to such high bending stress due to any slight misalignment or bending caused by "hogging" and "sagging" of the vessel, and consequently the plummer blocks are not so heavily loaded and the wear and danger of heating of the latter is minimized.

A further desirable feature of the hydraulic floating frame is that the oil and a certain amount of air which is always entrained with it, serves to act as a dampener and reduces the noise to a negligible amount. In fact, the noise made by gears fitted with the floating frame is certainly not in excess of that produced by a reciprocating engine developing the same power, and such noise as is produced is of a very low pitch and is not of a disagreeable character.

Two of the most essential requirements in passenger service are absence of noise and freedom from vibration.

Fortunately, high-speed turbines with short blades do not produce any appreciable noise, whereas, the slow-speed turbines with long blades, which are necessary in direct-connected installations, produce a singing noise far in excess of that made by a reduction gear fitted with an hydraulically floated pinion.

In regard to vibration, the geared turbine has all the advantages ot the direct-connected turbine in the absence of reciprocating parts, and has the advantage that, owing to the slow speed of the propeller, the latter works under conditions so far removed from those where cavitation occurs, that practically all of the vibration due to the propeller is eliminated, as it has been long since recognized that the greater part of the vibration experienced in direct turbine-driven vessels is due to the fact that the propellers are worked in the region where cavitation is about to begin.

The propeller vibration which has been objectionable in a number of reciprocating engine vessels has been rightfully attributed to the change of angular velocity of the screw, and hence, since the turning moment and angular velocity are absolutely uniform in a geared turbine, this source of vibration is eliminated.

A vertical and longitudinal cross-section and partial plan view of one of the 3,000 horse-power reduction gears for the "Neptune" is shown in Fig. 4.

For the benefit of readers who may not be familiar with the general principle of the Westinghouse reduction gear, it may be briefly stated that this gear consists of the floating or movable frame *A*, which carries the pinion in rigid bearings, the frame *A* having freedom, however, to adjust itself in the horizontal plane perpendicular to a plane passed through the center of the shaft and the nominal center of the pinion.

In order to avoid having the weight of the pinion and pinion frame coming on the teeth of the gear and pinion, the frame *A* is supported on struts *BB* held in adjustable sockets *DD*, which permit the vertical distance between the center of the pinion shaft and the gear shaft to be accurately adjusted; and to prevent the radial component of the pinion thrust lifting the frame from between its thrust abutments, a second set of adjustable struts *CC* and accompanying adjusting screws *EE* are provided, thus preventing the pinion shaft from moving away or approaching the gear shaft, but permitting motion in the horizontal plane. The struts and floating frame are shown in detail in Fig. 5 and Fig. 6. Oil for lubricating the pinion bearings

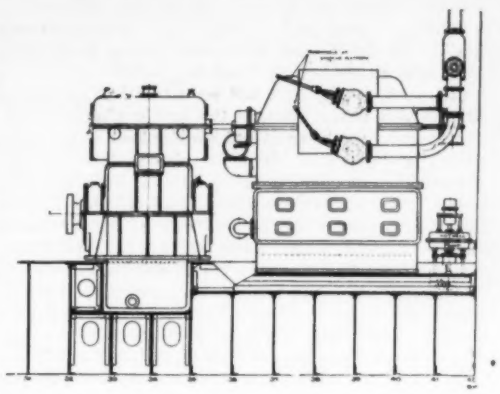


Fig. 7.—General fore and aft elevation of the arrangement of turbine and gears in the engine-room.

and pinion gear is supplied through the opening *H*, which communicates through a sliding contact *I*, Fig. 6, which communicates with the port *J*, extending the length of the pinion frame *A*, Fig. 5. A spray of oil floods the pinion through the ports *KK* and also supplies the bearings at the center line, or point of minimum pressure.

As has been previously pointed out, when a bearing rotates under load, unless metallic contact occurs, oil supplied at the point of minimum pressure of the bearing is carried around by the journal friction and the pressure of the oil film at the point where the load is carried must be sufficient so that the oil pressure times the bearing area is equal to the load on the bearing; otherwise the oil film would be destroyed and metallic contact would ensue as a result. By providing outlets at the point of maximum pressure, a bearing within the limits of its capacity may be employed as a pump, and this is what is done in the Westinghouse reduction gear, recesses being provided in the bearings at the point of maximum oil pressure in the bearings, which communicate with passages *L*, Fig. 6. A ball check valve *M* is provided in each passage *L*, through the bearings, and each of the passages *L* communicates through this check valve with a common space *N*, which is connected by passage *O* to the space behind the piston *V*. Now the total area of the pistons *V* is made approximately twice the projected area of the bearings, and hence an oil pressure of approximately one half that of the oil film in the bearings, if exerted on the pistons *V*, is sufficient to balance the thrust of the pinion; thus, as the pinion revolves under any given thrust, oil from the bearings at the pressure of the oil film in the bearings is pumped through the passages *L* to the space *N*, and through the holes *O* to the pistons *V*, balancing the movable or floating frame *A*, carrying the pinion in the direction of the arrow, or opposite to the direction of the pinion thrust, until the opening in the passage *E* in the arm of the floating frame moves away from the movable seat *Q* sufficiently to permit the excess oil pumped by the bearings to escape and thus relieve the pressure behind the pistons *V* until the product of the area of the pistons into the oil pressure is just equal to the total thrust on the pinion. Should the load increase, there is a tendency for the frame to be pushed over against the oil pressure behind the pistons *V*, and this brings the frame arm against the movable seat *Q* until the oil pressure behind the pistons again builds up sufficiently to balance the increased thrust; or if the thrust on the pinion decreases, the opposite action will take place. The seat *Q* is circular and has a shoulder which engages with the housing containing it, and it is held outward by a spring, thus forcing it in contact with the pinion frame arm until the frame takes the central position. This is made so that the frame normally when under no load can move back until the frame arm comes in contact with the gear housing. To prevent the pinion frame from tipping

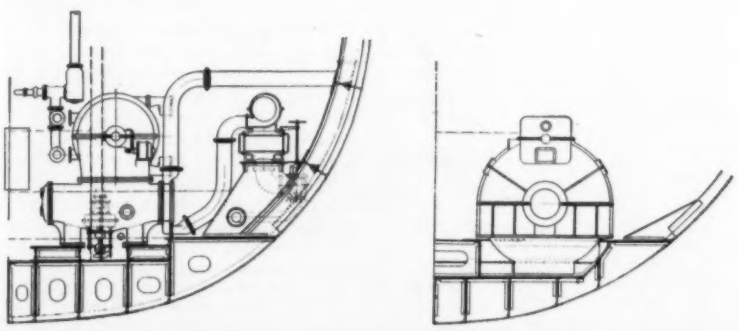


Fig. 8.—End-elevation through engine-room, looking aft.

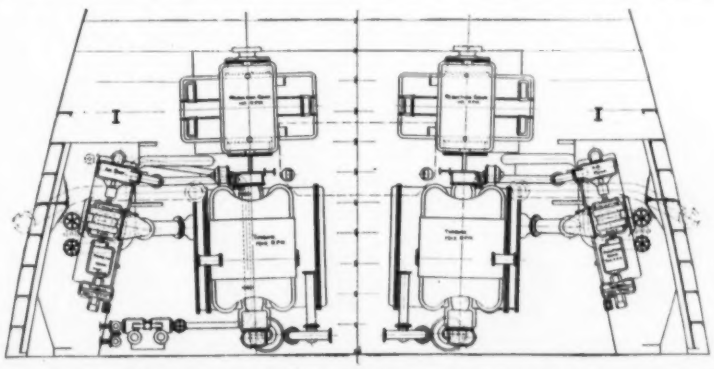


Fig. 9.—Plan of engine-room of "Neptune," showing location of air and circulating pumps.

when the frame arm comes in contact with the housing, a fulcrum *F* and fulcrum link *G* are provided which flexibly tie the floating or movable frame to the gear housing as shown in Figs. 5 and 6.

Referring to Fig. 5, it will be noted that the pinion drive which passes through the hollow pinion is connected to it by a taper fit and circular keys at the point *R* in the middle of the after pinion. This reduces considerably the total torsional deflection of the pinion under load. There are reasons, however, why it is not desirable to drive the pinion from a point between the two pinions, which would at first seem to be the obvious point to attach the pinion drive shaft.

The gear wheels are made of cast iron with cast steel rims pressed on, as shown at *WW*, Fig. 5. The steel castings from which the gear rims are made are cast two or three times the thickness of the finished rim and with a riser somewhat more than the width of the rim attached. So far, out of approximately 200 of these rim castings, not one has yet been machined which showed an imperfection at any point sufficiently large to be detected by the eye or by the aid of an ordinary magnifying glass. The material used for the rims is mild steel, having an ultimate tensile strength of about 60,000 pounds and an elastic limit of about 35,000 pounds.

Figs. 1, 2 and 3 show one of the pinions and floating frames in different stages of assembly. The lubricating grooves for the bearings and pinion spray are clearly shown in Fig. 1.

One of the pinions and half of its floating frame is shown in place on the gear in Fig. 2, while Fig. 3 shows the gear and floating frame assembled complete. The gages shown are for indicating the oil pressure behind the ahead and astern floating frame pistons respectively, and the pressure indicated on these gages multiplied by the area of the floating frame pistons in square inches gives the total effective thrust on the pinion, and this into the peripheral speed of the gear divided by 33,000 is the shaft horse-power transmitted. Experience has shown that the power transmitted is measurable by this means with an error of less than one per cent, so that the need of a torsion dynamometer on the propeller shaft is obviated; and, furthermore, by connecting the oil pipes from the floating frame to recording gages and using recording tachometers, it is possible to keep a continuous record of the shaft horse-power developed. A special form of differential pressure gage may also be used which will record the horse-power direct.

A general fore and aft elevation of the arrangement of turbine and gears in the engine room is shown in Fig. 7, in which it is seen that the turbine is mounted directly on the condenser as a bedplate. This arrangement with the pinion located on top of the gear materially reduces the floor space required and at the same time obviates the necessity of disconnecting and stowing the exhaust connections when it is necessary to remove the cylinder cover for inspection. A plan of the engine room of the U. S. S. "Neptune" is shown in Fig. 9, which shows the location of the air and circulating pumps. Fig. 8 is an end elevation through the engine room looking aft.

The Use of the Pitot Tube*

PRESSURE readings of 1 inch or less should be taken on an Ellison differential draft gage, graduated to at least hundredths of an inch, and calibrated by a hook gage; over 1 inch, either with an Ellison gage or with a U-tube graduated to at least tenths of an inch. Great care should be exercised to have perfectly tight connections, especially on the static side. A slight loss of pressure here will cause a correspondingly high velocity pressure reading, and therefore indicate too great a velocity.

The reading should be taken at a cross-section where the pipe is straight and the flow undisturbed, preferably at least 10 diameters from the fan outlet, from an elbow or from a change in cross-section in the duct. This condition is difficult of attainment in many installations and where it cannot be had the average of the maximum practicable number of readings at the point farthest away from the outlet or obstruction should be taken. Take the readings over a plane at right angles to, with the tube pointed in a direction parallel to, the direction of the air flow.

Pressure readings cannot be made over the face of an outlet with a pitot tube as is done with anemometer, but by inserting the tube in the pipe at the edge of the outlet with the tip of the tube pointed against the air flow, under the same conditions as in diagram *A*, a traverse of the pipe can be made at a point just before it reaches the outlet.

The most difficult reading to take accurately in a current of air is the static pressure. The approved form of static tip diagram *B* is that recommended for fan-testing work. There should be eight or more clean holes 0.02-inch in diameter, an equal number on each side of a ¼-inch tube 1/32-inch thick.

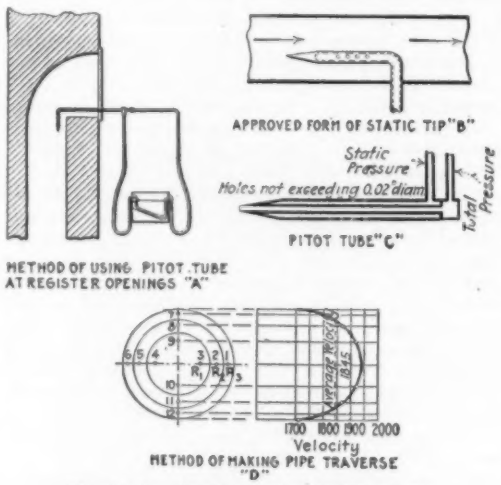
The most approved form of pitot tube combines the foregoing static tip with an impact tube, diagram *C*, by means of which total, static or velocity pressure may be read.

In making a traverse of a rectangular duct the cross-sectional area may be divided into a number of smaller rectangles and a reading taken in the center of each small rectangle.

* From report of committee, J. I. Lyle, chairman, at annual meeting of the American Society of Heating and Ventilating Engineers. Reproduced from *Power*.

A round pipe should be divided into at least three concentric zones of equal area per foot in diameter, and four readings taken on a circle drawn through the center of area of each zone or ring; that is, readings should be taken across the horizontal and vertical axis of the pipe, diagram *D*. Table I gives the distances from the center of the pipe to the points of reading, expressed in percentage of the pipe diameter. To get exact results a small pipe should be divided into relatively more zones than a pipe of larger diameter, as the ratio of frictional surface to cross-sectional area is greater; hence the more static pressure in proportion to the impact pressure, which correspondingly reduces the velocity pressure.

The corresponding velocities for each of these readings should be determined and an average found of all of these velocities to compute the air quantity. Inasmuch as the velocity varies as the square root of the pressure, accurate results cannot be obtained by averaging the pressure readings and taking the corresponding velocity as the average.


METHOD OF USING PITOT TUBE AT REGISTER OPENINGS "A"
APPROVED FORM OF STATIC TIP "B"
Static Pressure
Holes not exceeding 0.02" diam.
Total Pressure
PITOT TUBE "C"
1700 1800 1900 2000
Velocity
METHOD OF MAKING PIPE TRAVERSE "D"

Standard methods of using the Pitot tube.

TABLE I.—PIPE TRAVERSE FOR PITOT TUBE READINGS.

No. of Equal Areas in Traverse	No. of Readings.	Distance from Center of Pipe to Point of Reading in Per Cent of Pipe Diameter. 1st R.	2nd Rs.	3rd Rs.	4th Rs.	5th Rs.	6th Rs.	7th Rs.	8th Rs.
3	12	20.4	35.3	45.5	.....	.....	.....	.....	.....
4	16	17.7	30.5	39.4	46.0	.....	.....	.....	.....
5	20	15.5	27.2	35.3	41.7	47.4	.....	.....	.....
6	24	14.5	25.0	32.3	38.2	43.3	47.9	.....	.....
7	28	13.4	23.1	29.9	35.3	40.1	44.3	48.2	.....
8	32	12.5	21.6	28.0	33.2	37.6	41.5	45.1	48.4

The velocity may be determined from the velocity pressure by use of the formula

$$v = 1096.5\sqrt{\frac{p}{w}}$$

where

v = Velocity in feet per minute;
p = Pressure in inches of water;
w = Weight of air in pounds per cubic foot under the existing conditions of temperature, barometer and humidity.

With dry air at 70 degrees and 29.92-inch barometer

$$w = 0.0749$$

whence the formula becomes

$$v = 4005\sqrt{p}$$

With saturated air at 70 degrees and 29.92-inch barometer

$$w = 0.0735$$

and

$$v = 4046\sqrt{p}$$

Where only approximate results are desired, for circular pipe, multiply the velocity pressure taken at the center of the pipe by 0.81 or the velocity by 0.91.

For rectangular pipe, no definite factor can be given even approximately correct which will cover the varying proportions of width to height of the cross-sectional area.

Table II gives corresponding velocities for various velocity pressures of dry air.

The committee recommended the pitot tube as a simple and convenient instrument for the measurement of air or gases, which, when used with the proper care and accuracy of reading, gives results with an error of less than 1½ per cent, with velocity pressures ranging from 0.1-inch upward.

TABLE II.—CORRESPONDING VELOCITY FOR DRY AIR AT VARIOUS PRESSURES AND TEMPERATURES.

Pressure.		Temperatures, Deg. Fahr.									
In.	Oz.	50	60	70	80	100	150	200	300	500	550
0.1	0.0577	1242	1255	1266	1278	1300	1358	1413	1516	1704	1830
0.2	0.1154	1757	1776	1791	1808	1841	1921	2000	2145	2411	2590
0.25	0.1443	1965	1986	2003	2022	2059	2149	2235	2399	2696	2895
0.3	0.1730	2151	2175	2193	2214	2254	2352	2447	2626	2952	3175
0.4	0.2308	2485	2512	2533	2557	2603	2717	2827	3033	3409	3660
0.5	0.2884	2778	2808	2832	2859	2911	3038	3160	3391	3812	4095
0.6	0.3460	3043	3076	3102	3131	3188	3327	3462	3715	4175	4490
0.7	0.4037	3287	3323	3351	3383	3445	3595	3740	4013	4510	4850
0.75	0.4326	3402	3439	3468	3501	3565	3720	3870	4153	4668	5020
0.8	0.4614	3524	3552	3582	3616	3682	3843	3997	4290	4821	5185
0.9	0.5190	3728	3768	3800	3836	3906	4076	4221	4550	5114	5500
1.0	0.5768	3929	3971	4005	4043	4117	4296	4470	4796	5390	5795
1.25	0.7209	4393	4440	4478	4520	4602	4804	4997	5362	6027	6470
1.50	0.8650	4812	4864	4905	4952	5042	5262	5474	5874	6602	7100
1.75	1.0092	5197	5254	5298	5348	5446	5683	5912	6344	7131	7655
2.00	1.1535	5556	5616	5664	5718	5822	6076	6320	6783	7624	8195
2.25	1.2975	5892	5956	6007	6064	6174	6443	6704	7193	8085	8690
2.50	1.4418	6211	6278	6332	6392	6508	6792	7066	7582	8523	9150
2.75	1.5860	6514	6585	6641	6704	6827	7124	7412	7952	8938	9600
3.00	1.7300	6807	6879	6937	7003	7130	7440	7742	8307	9336	10000
4.00	2.3070	7857	7942	8010	8086	8233	8592	8940	9581	10780	11580
5.00	2.8840	8772	8867	8943	9027	9192	9593	9980	10710	12037	12900
6.00	3.4600	9623	9728	9810	9903	10083	10523	10950	11750	13203	14180

Microbian Droplets

M. Roux has summed up the works of M. Trillat, of the Pasteur Institute, and M. Tanassier, concerning the formation of microbian droplets in the atmosphere and their properties. The authors have first shown that microbes can play the rôle of a nucleus for the condensation of humidity in the air, which explains the presence of numerous microbian drops, which are so much the lighter as the germ inclosed is smaller. The speed of the fall of these droplets follows the law of Stokes. MM. Trillat and Tanassier have studied the action of cooling on these microbian droplets. Whereas the total cooling of a microbian mist, like that of a sudden decompression, has the effect of resolving the drops into rain and of purifying the atmosphere, the lowering of the temperature of one point of a microbian atmosphere determines an immediate transport of the germs that assemble and are localized in the cooled regions. The case is striking for cooling surfaces. The knowledge of these results throws some light on the transmission of certain diseases by the intermediary of microbian droplets. Perhaps, in the future, some applications may be made for the purification of rooms, halls, hospital wards, etc.—*Chemical News.*

Forest Fires in the United States have caused an average annual loss of 70 human lives and the destruction of $25,000,000 worth of timber.

Filtration of Potable Waters*

The Paterson Rapid System for Large Scale Operation

THE first record of a slow sand filter installation, of the type now so extensively used, seems to be that of one laid down by James Simpson at the Chelsea Water Works in 1829. Since that date slow sand filtration has been recognized as the most effective method of securing a pure public water-supply from contaminated sources. But while water engineers may be said to be agreed that the slow sand filter is a reliable and efficient device for attaining this result, it must be admitted that it has definite limitations, and that recent developments in rapid mechanical filtration are now advancing their claims to high efficiency.

It is recognized that filtration through slow sand-beds of sound construction, operated under efficient supervision, secures a degree of purification leaving little to be desired beyond the elimination of the disadvantages inherent in this system—namely, the inability to remove discoloration due to fire-clay, peat stains, and other causes. These cannot be removed on a large commercial scale without chemical treatment, and water so treated must not be passed directly on to slow sand-beds owing to the rapidity of choking up, which necessitates repeated paring of the surface film and washing. The principal screen for the interception of suspended matter and bacteria in the slow sand filter is this surface film, consisting of algoid and other vegetable growths, and requiring considerable time to form. In order to maintain the required output it is necessary to duplicate installations of moderate size, so that the cleaning may be effected without hastening the rate of filtration in the active beds; but this entails a large capital outlay and considerable waste of water for sand-washing and film formation. With the rapid filtration system these washing and filtering processes occupy only a few minutes, and by the Paterson method of compressed-air cleaning the amount of wash-water required is reduced to a minimum, as the water is not used during the agitating process, but only to wash the loosened impurities to the drain.

For satisfactory working it is essential that the surface film be uniform, otherwise the rate of percolation will vary in different parts of the bed. With the slow sand filter, weather conditions may effect the uniformity of the filtering film, and it is frequently injured by burrowing earthworms and insect larvæ, which perforate the film and allow the passage of unfiltered water. The rapid filter is free from this defect, as its film is composed of pure inorganic matter—hydrate of alumina. Slow sand filtration effects no appreciable reduction in coloring matter, and fails to remove peaty acids, which are found in many moorland supplies, and which, owing to their distinctly plumbo-solvent powers, constitute a source of danger.

The rapid filtration system under review claims to effect in a convenient manner the complete elimination of all discoloration, the neutralization of natural acidity, and the removal of suspended matter and harmful bacteria. Mechanical filters installed in this country (England) have, for the most part, been of the vertical-pressure type, consisting of a steel receiver for the filtering medium, which medium is periodically agitated by various types of mechanical stirrers, the application of the coagulant being effected by means of pumps driven by turbines in the supply main, the water, after treatment, passing directly on to the filter-beds.

The Paterson standard practice differs essentially from this, however, in so far as (*a*) the gravity system is adopted; (*b*) the filtering surface is not broken up by mechanical agitators; (*c*) two or three hours are allowed for the reaction to complete before the water passes on to the filters; (*d*) the rate of filtration is much lower, as the filtrating area is more than double the recognized standard of existing rapid filters. The process, moreover, is carried out in three distinct stages—namely, (1) the accurate measuring of the water and the addition and mixing of the coagulant (and, where necessary, sterilizing reagent) in precise ratio to the amount of water passing; (2) the subsidence of the treated water in a reaction and precipitating tank of sufficient capacity to allow the completion of the chemical reaction and the precipitation of the grosser impurities; (3) the final purification by rapid filtration through quartz sand.

It is maintained that gravity filtration is preferable to the pressure type for the following reasons: (1) The gravity filter is open to inspection, and the condition of the filter-bed and the efficiency of the cleaning process can be seen at all times. (2) The gravity filter works with a natural head, and cannot be influenced by variations of pressure in the supply main.

The Paterson plant at the Cheltenham Water Works, at Tewkesbury, effects the purification of the supplementary water supply, amounting to 1,000,000 gallons a day, pumped from the River Severn. The City of Worcester, fourteen miles higher up the river, at present discharges its crude sewage into the Severn, making it a highly polluted source of public supply. Between Worcester and Tewkesbury, however, a marked natural purification takes place, but the analysis (given later in this article) shows that it is still very much polluted. The river at times of heavy flood is very turbid and highly discolored from the washings off the land and the peat and decaying vegetable matter from the upper reaches of the stream. As a result the slow sand filters previously installed at Tewkesbury choked up rapidly, and at times required scraping every day or two, entailing a heavy expense for labor, the waste of a large quantity of filtered pressure-water for sand-washing, and causing grave anxiety to those responsible for the maintenance of a pure supply.

In the spring of 1909 the problem reached an acute stage, and tenders were invited from various filter-makers, but at that time no proposal was considered quite satisfactory under the special conditions. In 1911, however, a deputation, consisting of the Mayor of Cheltenham, the borough surveyor, the medical officer of health, and the eminent authority, Dr. Rideal, visited the installation of Paterson's rapid filters, installed at the works of the Weardale and Consett Water Company. The impression formed was favorable, and it was at once decided to order a similar apparatus, with the addition of chemical treatment.

Having referred generally to the installation, we will now, with the aid of the illustrations accompanying this article, describe more in detail its actual construction. The water pumped from the River Severn enters the automatic measuring and chemical supply tank shown at *A* in Figs. 1, 2, 4, and 6, and discharges through two V notches, which accurately gage the quantity of water passing (see Figs. 4 and 6). A float *B* riding on the surface of the water controls the position of two tapered valves *C* and *D*, Fig. 6. They are, however,

* Reproduced from *Engineering*.

Fig. 1.

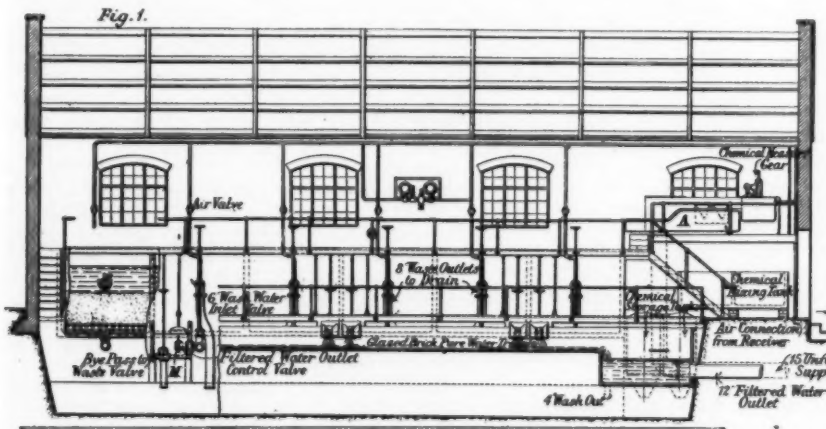


Fig. 2.

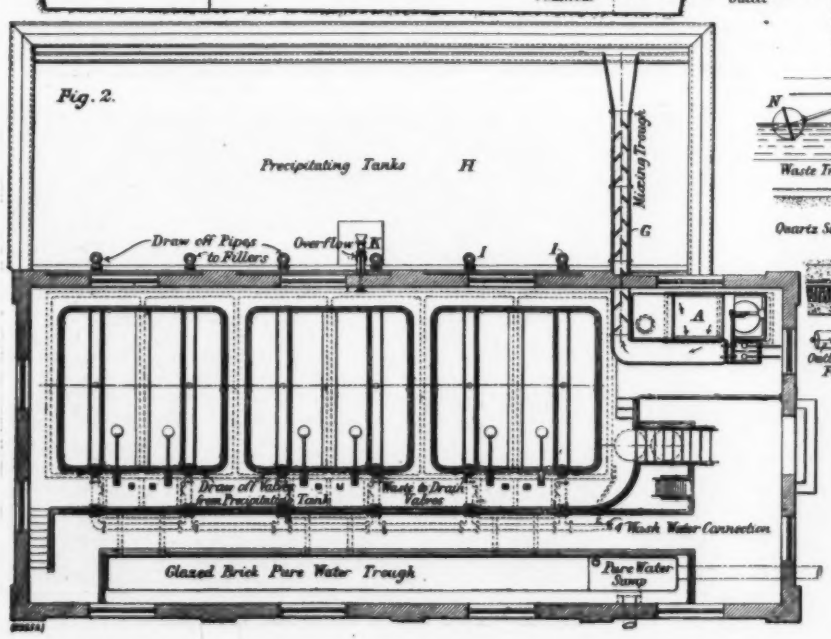


Fig. 3.

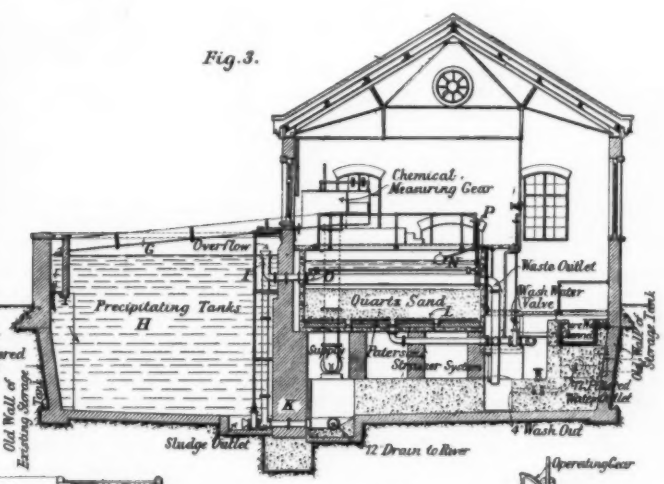


Fig. 5.

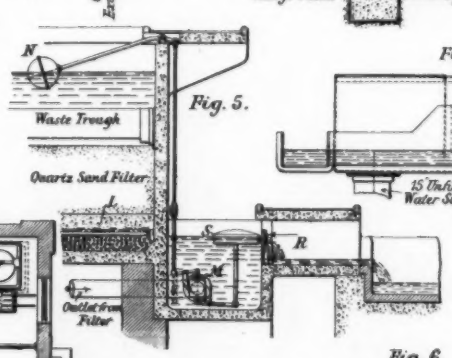


Fig. 4

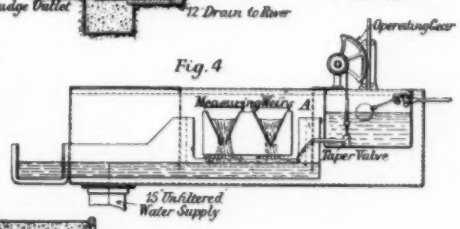


Fig. 6.

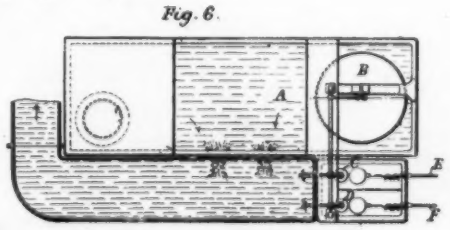


Fig. 7.

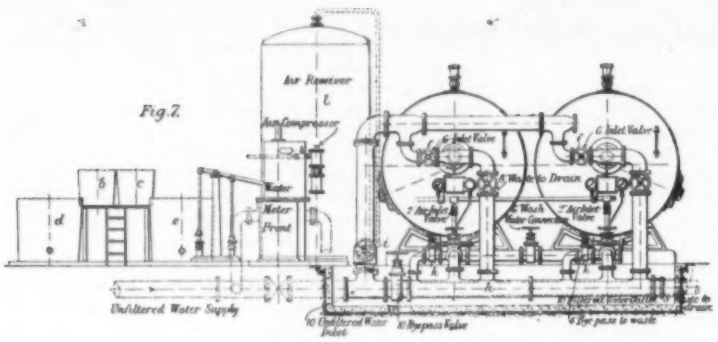
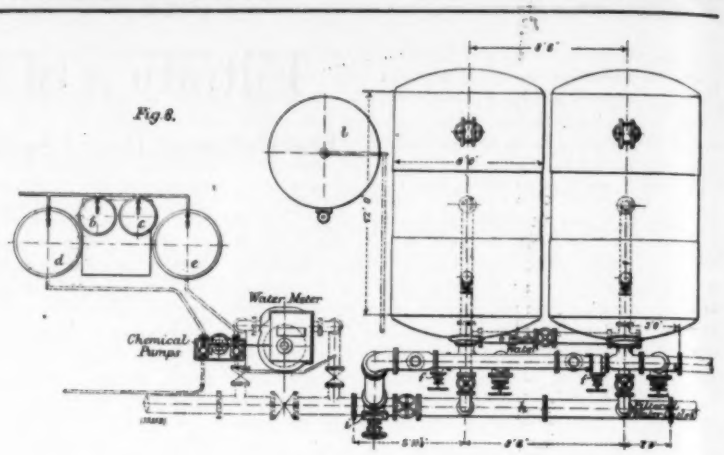


Fig. 8.



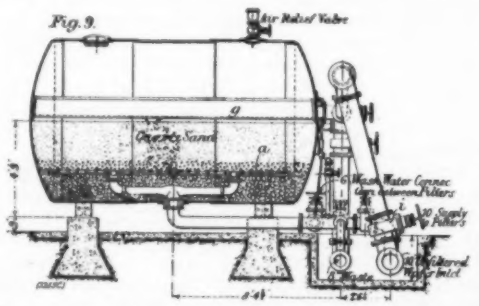
shown in detail in Figure 10. These valves give annular orifices for the supply of the reagents in the precise ratio to the quantity of water delivered. The reagents (sulphate of alumina for the coagulation of the impurities and extraction of the coloring matter, and hypochlorite of lime for sterilization when required) are maintained at a constant head above the valve openings by ball-cocks *E* and *F*, Fig. 6, which are connected to the pressure storage tanks in the basement by means of pipes. The contents of the chemical tanks are continually agitated by compressed air, which also supplies the pressure required to lift the reagents to the controlling apparatus referred to. The chemical solutions are prepared in concrete tanks on the filter-house floor-level, the necessary agitation and mixing being performed by compressed air.

As the water passes from the tank *A* it comes in contact with the reagents, with which it is thoroughly mixed as it passes down the mixing trough *G*, Figs. 2 and 3, which discharges into a reaction and precipitating tank *H*, having a capacity equal to about three hours' pumping, allowing time for the complete reaction of the alumina, and the precipitation of the grosser impurities. The partially purified water over-flows through bell-mouthed draw-off pipes *I* into six Paterson rapid gravity filters, as shown in the illustrations. These filter-tanks are made of reinforced concrete, and contain 36 inches of special-grade quartz-sand filtering medium resting on a stratum of pea-gravel. In each of these filters there is a centrally-placed inlet distributing and a waste draw-off channel *K*, Figs. 2 and 3. The filtered water is collected from underneath the gravel by the system of gun-metal strainers shown at *L*, Figs. 3 and 5. These have screwed caps and renewable phosphor-bronze screens, and they discharge through the automatic outlet controller into the inspection-box, from which the water passes into the clear-water channel. All the valves and fittings are so arranged that the entire filtering and washing operations are controlled by one attendant from the main gallery.

In a demonstration of the cleansing process the filter-bed was first agitated with sterilized compressed air at a pressure of about 5 pounds per square inch, and the impurities, loosened by this air scouring operation, afterward floated to waste by a reverse current of wash-water. The process appeared to be particularly efficient, and the filter unit was thoroughly cleaned and restarted within 10 minutes. Particularly striking was the agitation effected by the compressed air, every part of the bed, even the corners, being agitated to the same extent. As the wash-water was turned on, it forced the air out of the system, and so ensured agitation up to the moment when the loosened particles were floated into the waste channel.

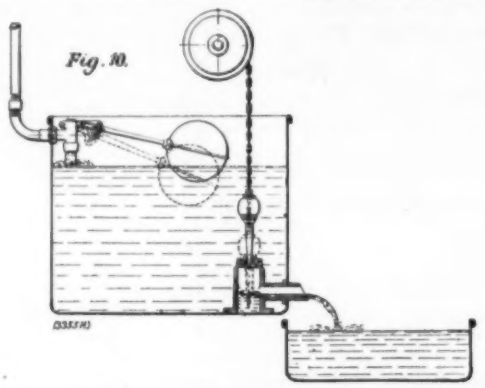
After the cleansing had been carried out, and while the surface film was forming, the filter was prevented from coming into full operation until from 10 to 15 minutes had elapsed, by means of the automatic controller, shown at *M*, Fig. 5. This appliance fulfils a very important duty. It is in the form of an equilibrium valve under double control and acts in the follow-

Fig. 9.



ing way: When the cleansing process has been completed, the water above the filter-bed is, of course, at the level of the waste-trough, and the outlet-valve is held down in its closed position by the weight of the float *N*, Figs. 3 and 5. When the filtration process recommences, however, the inlet valve *O*, Fig. 3, is opened by means of the hand-wheel *P*, and the filter gradually fills up,

Fig. 10.



but the outlet controller *M* remains closed until the level of the water reaches the float *N*. The outlet valve gradually opens as the float rises until it is released from the restraint of the float, and the rate of discharge is then controlled only by the regulation-float in the inspection box. This inspection box is fitted with an overflow weir in the form of a V notch *R*, Fig. 5, the arrangement being such that when the water, before flowing through the weir, rises to the height determined by the maximum allowable rate of discharge, the regulating float *S* partially closes the outlet valve, so that this maximum rate of filteration cannot be exceeded. By means of this outlet-controller the following conditions on which successful filtration depends are secured: (1) It allows time for filling, and then starts the filter gradually, after the cleansing process is complete, so ensuring that all water entering the service main is of the required degree of purity. (2) It governs the rate of outflow, so that the maximum safe rate cannot be exceeded. (3) It steadies the flow and ensures a uniform rate of filtration. (4) It prevents furrowing of the filtration surface by ensuring that a protective layer of water is always maintained above the sand bed when the supply, for any reason, is very much below the normal, or, indeed, is temporarily shut off

At the time of the writer's visit to Cheltenham the river water appeared to be very turbid. When viewed through a 2-foot tube it showed a muddy discoloration, and was quite opaque. After filtration the water was bright, clear, and colorless, and the smallest print could be read through the water in a 2-foot tube. By means of a Lovibond's tintometer, in which the color of the water in a narrow trough 2 feet long is compared with a standard white light, and graduated colored glasses interposed between the eye and the light until the tint of the sample is exactly matched, the color of the water entering the filter was found to be represented by red, 7; yellow, 10.5; and blue, 5.7 a total of 23.2, while the filtered water was practically colorless, the amount of tint present being almost too minute for expression by the standard scale of glasses available. The lowest—0.1 blue and 0.1 pink—most nearly approximated to the sample under examination—a percentage removal of over 99 per cent. On March 31 the river water gave a reading of red, 10.5; yellow, 18.0; blue, 8.5—total,

—	Untreated Water.	Treated Water.
Physical examination..	This was highly polluted and unfit for drinking purposes; it gave a brown sediment on standing and had a slight marshy smell	This showed a very marked improvement, chemically, bacterially, and physically, being clear, colourless, inodorous, and tasteless.
Chemical examination..	Parts per 100,000	Parts per 100,000
Dissolved solids..	40.92	42.72
Suspended solids	1.97	nil
Chlorine..	5.28	5.92
Free ammonia	0.0040	0.0045
Albuminoid ammonia..	0.009	0.004
Nitrates (as nitrogen)..	0.088	0.100
Nitrites	Heavy trace	nil
Oxygen consumed	0.23	0.115
Bacterial examination..	—	—
Total organisms	2500 per c.c.	Less than 1 per c.c.
Coli organisms	50 (or more) per c.c.	Entirely absent from 100 c.c.

Fig. 11.—Upper view in filter-room, Tewkesbury.

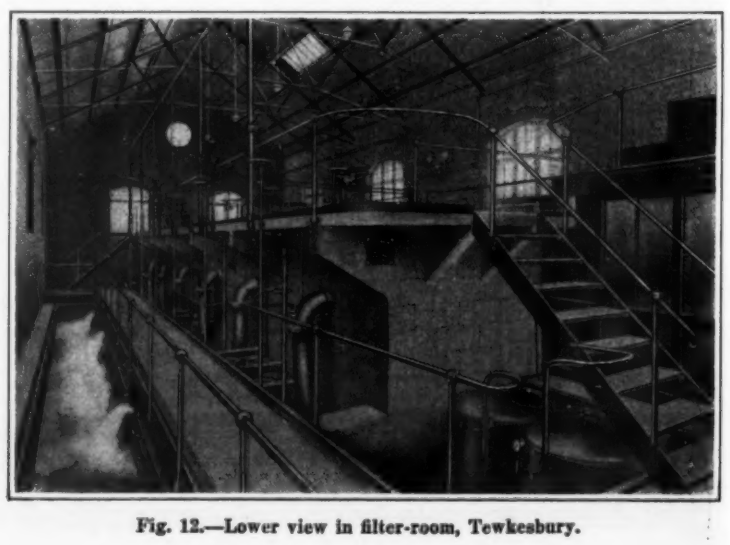


Fig. 12.—Lower view in filter-room, Tewkesbury.

37.0; the filtered water was colorless. With regard to the chemical and bacterial quality of the water, the foregoing particulars are of interest, having been derived from an independent analysis carried out by Dr. Samuel Rideal on samples of water taken from the plant we are describing, in September, last year.

From this it will be noted that a very high degree of purification (well over 99 per cent) has been attained on all three headings of discoloration, suspended matter, and bacteria.

As will be seen by the illustrations, the whole installation is very simple in design, and all the working parts are particularly easy of access. All the flooring and platforms are laid with "Terrazzo," and are edged with green glazed bricks, the general appearance being very pleasing. The general arrangement is shown in Figs. 11 and 12, opposite page, which are reproductions from photographs. The pure-water collecting-channel and the inspection-boxes are lined with white glazed bricks, and as the water is at all times viewed through a depth of 3 feet or 4 feet, the slightest discoloration is at once apparent to the attendant in charge. It may be mentioned here that the complete installation was erected in what was formerly the crude-water receiving-tank, which accounts for the rather peculiar construction of the lower half of the subsidence reservoir, as may be seen in Fig. 3.

Although the builders of the system here described strongly recommend the adoption of gravity filtration for the reasons already mentioned, they appreciate that there are circumstances when the pressure type of filter is more convenient, as the water can be drawn from a river or other source and pumped through the filters into a reservoir at a higher level, which saves re-pumping, and under some circumstances makes the installation of the pressure filter an advantage. In this case, where the purified water is intended for a domestic supply, an exceptionally large filtering area is advocated, in order to keep the rate of discharge per square foot of area at a low figure. With the exception that the filtering medium is contained in a closed vessel capable of withstanding the pressure, the working of this type of filter is very similar to the gravity type already described. It will be noted from the following description, and from accompanying illustrations, that the characteristic feature of the Paterson gravity filter is maintained in so far as the top film of sand is unbroken by any form of mechanical agitator, and the waste water is drawn off by a central collecting-trough within 18 inches of the level of the sand bed.

In Fig. 3 may be seen a perspective view of two of these pressure filters, constructed for the India Office, for the purification of the water supply to the military camp at Chidambaram, amounting to half a million gallons a day. Fig. 14 is a view of the metering and chemical-supply gear, which we will presently describe. The filters are also shown in Figures 7 to 9, Fig. 7 being a front elevation, Fig. 8 a plan and Fig. 9 a vertical longitudinal section through one of the filters.

Each filter is in form of an 8 foot diameter steel riveted shell, 14 feet long, carried on two cast-iron stools. This arrangement enables the filtration units to be constructed with a very large filtering surface, and very materially reduces the cost and the amount of attention required—matters of very considerable importance in large installations.

The filtering medium consists of about 30 inches of quartz-sand resting on about 6 inches of pea-gravel. The purified water is drawn off by the Paterson system of gun-metal strainers with screw caps, which hold securely in position the removable perforated bronze screens. The arrangement is clearly shown in Fig. 9. The central channel *g* running the full length of the filter effects the distribution of the incoming water during the filtration process, and the withdrawal of the waste during the cleansing process. The pipework has been very carefully and conveniently arranged, and bears evidence of having had much thought bestowed upon it.

In connection with pressure filters, one of the difficulties has been to obtain a thoroughly reliable means for effecting the addition of the coagulant in exact ratio to the amount of water passing. In the plant we are describing a large reciprocating meter of the Kennedy type is made to operate a plunger-pump, which forces in a definite weight of the necessary coagulant in exact proportion to the flow of water as metered. The arrangement is shown in outline in Figs. 7 and 8, and in perspective in Fig. 14. It will be noted that there are two pumps in the arrangement shown, one of which is for coagulant, and the other for the hypochlorite of lime. These pumps draw their supply from two storage tanks *d* and *e*, on the top of which are two small preparing-tanks *b* and *c*, Figs. 7 and 8. By this arrangement a positive operating force is available for pumping in the reagent under pressure in the exact proportion required, and which has in practice been found to be very simple and satisfactory.

The impure water, after being treated with the necessary coagulant (and, if required, sterilizing reagent), passes through the inlet-valve *f*, Figs. 7 and 8, into the distributing channel *g*, Fig. 9. and after filtering through the quartz bed, is drawn off by the gun-metal strainers *a*, and discharged through the outlet-valve into the filtered-water main *h*. The choking-up of the filter-bed with impurities is indicated by the gradual rise in the difference of pressure between the inlet and outlet sides of the filter, as indicated by the pressure-gages. When this difference has reached the maximum permissible—from 5 pounds to 10 pounds per square inch—it is time to cleanse the filter.

This operation is effected in about 10 minutes by, first closing the inlet-valve, then opening the waste-valve and draining the water down to the level of the waste-trough. On opening the air-valve, sterilized compressed air at about 5 pounds per square inch enters the strainer system, and by the special arrangement of the distributing pipes and strainers unseals the orifices of all the gun-metal strainers at the same time, so that an equal distribution of air takes place over the whole filter-bed. After agitating with compressed air for two or three minutes, the loosened impurities are flushed over into the central waste channel by a reverse current of cleansing water and flow away to the drain. On the completion of this process, the waste-valve is closed and the inlet-valve at *f* is opened and the filter gradually fills up and the filtering process recommences. A by-pass valve is fitted at *k*, which connects with the outlet from the filters to the waste-drain, so that the first filtered water can be run to waste for about five minutes.

The sterilized compressed air is stored at a pressure of about 50 pounds per square inch in an automatic receiver *l*, Fig. 7, the air being pumped in by a steam-driven air-compressor, fitted with a cut-off valve which comes into action when the air in the receiver has attained the desired pressure. It will be seen, therefore, that the compressor is only required occasionally for the recharging of the receiver. The compressed air is filtered free from grit, oil, or other impurities and sterilized before entering the air-receiver.

In conclusion, the advantages claimed for this system may be summarized as follows: (1) The central channel ensures the equal distribution of the incoming water, prevents furrowing, and draws off the impurities uniformly during the cleaning process. (2) The compressed-air and wash-water agitation thoroughly scours the sand grains, and keeps the bed fresh and wholesome by repeated aerations. (3) The lowest possible proportion of wash-water is required, averaging 1 or 2 per cent of the total volume purified. This result is due to the fact that the wash-water is not required during the air-scouring process, but only afterward, to float the loosened impurities to the drain. (4) The construction of the manifold strainer system ensures uniform agitation of the filter-bed by the compressed air and wash-water. These cleansing agents reach every part of the filtering medium with equal force, as is obvious from inspection of one of the gravity filters during the cleansing process. (5) The liberal filtering area provided (more than double the recognized standard for existing rapid filters) enables the rate of filtration to be kept at a low figure. (6) The filters are simple, compact, and so arranged that all valves are operated from the main working platform.

It would appear that the slow sand filter has a formidable rival in this latest development of the rapid mechanical filter, which, in regard to the sterilization of the water and the complete removal of color, effects a result which has never yet been attained by slow sand filtration. Indeed, these gravity filters at the Cheltenham Water Works were, we understand, originally put down by the engineer in order to give the Severn water rough purification before it was passed through the slow sand filters, but it was found that when the Paterson filters were installed they purified the water alone to a higher extent than was found possible on the slow sand filters.

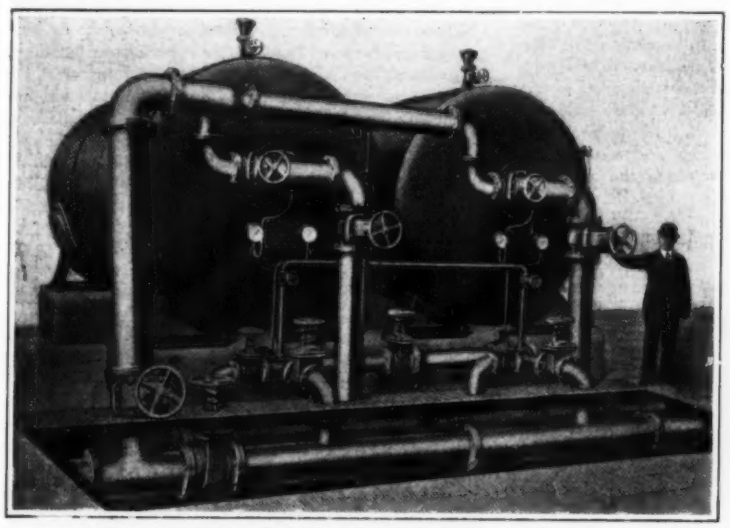


Fig. 13.—Pressure filters.

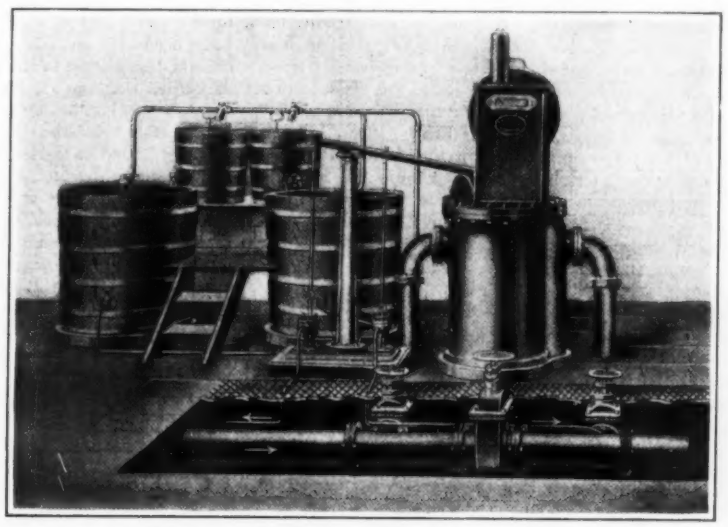


Fig. 14.—Metering and chemical supply gear.

The World's Production of Petroleum in 1913

According to statistics that have been published for the first three quarters of the year 1913 concerning the exploitation of the different petroliferous regions of the whole world, it is seen that all the records established hitherto have been beaten. A comparative statistic of the last five years shows that the world's production has gone on steadily increasing. In 1912, the countries producing petroleum supplied 351,178,236 barrels of 42 gallons each, which makes 3,190,383 wagons of petroleum of 10,000 liters each. The increase which the petroliferous production of 1913 will show will certainly be due to the formidable gain registered by the output of California and of the naphtha fields of Kansas and Oklahoma. It is the United States that holds the first place among the countries producing petroleum. Their annual production exceeds 220 millions of barrels, and is 64 per cent of the whole world's production. Russia comes next, with 70 millions of barrels and a percentage of 19.37. These two countries alone represent, then, about 82 per cent of the petroliferous production of the whole world. The third place is held by Mexico, a country that has only just lately come to the front as a producer of petroleum. The part it contributes is 4.71 per cent. Roumania and the Dutch Indies are closely matched for the fourth and fifth places, with a rate of 3.70 per cent and 3.09 per cent in the naphthiferous production of the world. Still another country on which geologists found great hopes for the future in what concerns petroleum is the Argentine Republic, the naphtha beds of which have up till the present time been very little explored.—*Chemical News.*

Paraguay has Valuable Forest Resources, the most important of which is quebracho, particularly rich in tannin.

The Number and Light of the Stars*

Is the Universe Finite?

By S. Chapman

THE number of stars visible to the naked eye on a clear night, in the whole sky, is roughly 5,000—a very moderate total indeed, in spite of the universal custom of using the number of the stars, in common with that of the sands of the sea, as synonymous with infinity. In all ages, mankind, in general, has rightly preferred rather to admire and wonder at the stars than to count them. In the great problem of the structure of the sidereal universe, however, which astronomers are now attacking with much energy and success, one of the essential data is the number of stars in the different regions of the sky, classified according to their brightness or magnitude.

The telescope early revealed the fact that the stars, which by reason of their superior brightness force themselves upon our unaided vision, shine forth beyond a host of fainter luminaries, the number of which has never yet failed to show an increase as an augmentation of telescopic or photographic power has enabled us to pierce depths of greater and greater obscurity. These fainter stars enormously outnumber the naked-eye stars, which may be compared with an oceanic island, the tiny, outstanding peak of a great mountain growing ever broader beneath the water level.

The bright stars have been divided into six traditional classes or magnitudes, the brightest being called first magnitude stars, and the faintest visible being termed of the sixth magnitude. Pogson placed this classification on a scientific basis, so that it could be extended to telescopic stars of all degrees of faintness. On the conventional standard scale the ratio of the intensities of two stars differing in magnitude by the amount m is $10^{-0.4\,m}$. The determination of the magnitude of a star on this scale is not an easy matter, as it involves the measurement of the relative brightness of stars often differing much in luminosity.

Photographic methods have been found most suitable for the purpose; the results are rather different from those obtained by visual methods, especially for stars of different color, since the photographic plate is more sensitive than the eye to blue rays, and less sensitive to red. Magnitudes determined by ordinary photographic methods are called "photographic magnitudes."

During the past three years, the fine series of star charts obtained by the late J. Franklin-Adams has been used in the investigation of the number of stars of determined photographic magnitudes in all parts of the sky. Franklin-Adams, using a specially designed Taylor-Cooke 10-inch lens covering a wide field, photographed the whole sky on 206 plates each 16 inches square, the scale being 20 millimeters to 1 degree; the photographs were taken (the northern set in England, the southern in South Africa) with exposures of from two to two and a half hours, which sufficed to show stars down to the seventeenth magnitude on most of the plates. From a star of this magnitude we receive only one-millionth as much light as from a second magnitude star. On each plate, the stars in 25 uniformly distributed sample areas have been counted and classified according to the size and grayness of their images; altogether, therefore, sample counts have been made on more than 5,000 regions of the sky, at intervals of 3 degrees apart. The areas examined were of different sizes, being chosen, in accordance with the star density in the particular region, so that about 60 stars should be counted in each. The stars could not be classified according to their photographic magnitude directly from the plates, as stars of different magnitudes might show images identical in size and grayness, on different plates, owing to inequality of the atmospheric transparency during the exposures on the two plates. The photographic magnitudes of a selection of the stars counted on each plate needed, therefore, to be determined directly, which was done by comparing them with the stars of the North Polar Sequence, a set of stars the magnitudes of which have been given very accurately by Prof. E. C. Pickering, of Harvard. Auxiliary photographs for this purpose were taken with the 30-inch reflector at Greenwich. After the application of corrections depending on their position on the Franklin-Adams plates it was then possible to classify all the stars counted, upon a true magnitude basis. This complete reduction has so far been effected for 30 out of the 206 plates counted; the results from the 750 areas for which sample counts were thus afforded have recently been published.[1]

The first point of importance which will be mentioned as one on which the evidence derived from this work is decisive is that of the relation between the condensation of the stars toward the galaxy and their magnitude. While the very brightest stars show little regularity of distribution (to their irregular grouping, indeed, much of the beauty of the constellations is due), the fainter naked-eye stars show a distinct concentration toward the plane of the Milky Way, their density in the galactic belt of the celestial sphere being about twice that near the poles of the Milky Way; still fainter stars down to the ninth magnitude show this phenomenon in a more marked degree, the density of these stars in the galaxy being three or four times that at the galactic pole. The galactic plane is a fundamental one in modern representations of the sidereal universe, which, following Sir W. Herschel's ideas, picture the stellar system as formed of a large central cluster of stars in the form of an oblate spheroid, equatorially surrounded by a belt of irregular star-clouds composing the Milky Way. In connection with such a theory it is of obvious importance to know whether the condensation toward the galaxy shown by the stars of magnitudes five to nine persists for still fainter stars, and in what degree. Very different views have been held on this matter. In regard to the stars classified according to their visual magnitudes, Kapteyn concluded that the galactic condensation increases very much with diminishing brightness, giving its value for all the stars brighter than 17^m as 45. Pickering, on the contrary, found no marked change in the relative densities in the galaxy and at its poles, down to the thirteenth magnitude (the limit of his data). The counts on the Franklin-Adams plates, based on a photographic magnitude classification, lead to a similar result; the density of all the stars brighter than 17^m in the galaxy does not exceed six times that at the galactic poles, and the ratio is perhaps not more than four. Although it is possible that there may be a systematic change of color of the stars with increasing faintness, different in different galactic latitudes, which would make results derived from counts of stars of determined *visual* magnitudes differ systematically from those based on photographic magnitudes, yet such evidence upon the point as already exists renders it probable that the galactic condensation is in either case nearly constant from the sixth to the seventeenth magnitude.

On this account the rate of increase in the number of stars per magnitude will be nearly constant all over the sky, so that this rate may conveniently be studied from a table giving the numbers of stars in the whole sky brighter than each magnitude m; this will be denoted by N_m. All the best available data have been embodied in the following table, giving N_m for values of m down to 17. The values of $\triangle_m (= \log N_{m+1}/N_m)$ are also given, as they provide a measure of the geometric ratio of increase in the number of the stars.

TABLE I.—*The Number of Stars in the Whole Sky Brighter than Magnitude m.*

m	N_m	$\log N_m$	$\triangle_m$
2	38	1.58	....
3	111	2.05	0.47
4	300	2.48	0.43
5	950	2.98	0.50
6	3,150	3.50	0.52
7	9,810	3.99	0.49
8	32,360	4.51	0.52
9	97,400	4.99	0.48
10	271,800	5.43	0.44
11	698,000	5.84	0.41
12	1,659,000	6.22	0.38
13	3,682,000	6.57	0.35
14	7,646,000	6.88	0.31
15	15,470,000	7.19	0.31
16	29,510,000	7.47	0.28
17	54,900,000	7.74	0.27

The data for the stars of magnitudes 2 to 6 are somewhat uncertain, which accounts for the irregular run of the first few values of $\triangle_m$, but beyond this point the steady decrease in $\triangle_m$ is very noticeable. This clearly shows that modern photographic telescopes now penetrate to regions of space where the stars begin to thin out in numbers to a quite considerable extent, for it is easy to prove that if the stars were distributed uniformly throughout space, $\triangle_m$ should preserve the constant value 0.6. This assumes, what appears to be fairly correct, that any possible absorption of light in space does not materially diminish $\triangle_m$.

From the numbers in the foregoing table, the following simple rational formula can be derived,

$$\log \frac{dN_m}{dm} = a + bm - cm^2$$

or, in an equivalent form,

$$N_m = A \frac{1}{\sqrt{\pi}} \int_{-\infty}^{B(m-C)} e^{-x^2} dx$$

The latter formula, which is the integral of the error curve, implies that the total number of the stars is finite, and this is now generally accepted as true; A represents this total number, while C denotes the magnitude which divides all the stars into two equal groups, those brighter being equal in number to those fainter. A, B, C can be deduced from a, b, c, which are readily obtained from the observed values of N_m, but A is not narrowly determined—its value seems to be not less than 1,000 million, and probably not greater than 2,000 million, so that the total number of the stars is comparable with the population of the earth (this is roughly estimated as 1,600 million). The constant C is more closely determined, and is approximately 23 or 24. Stars of this magnitude could just be photographed, with many hours exposure, with the largest telescope in the world, the 60-inch reflector at the Mount Wilson observatory. There remain, therefore, beyond our present powers of exploration still fainter stars equal in number to all those which could possibly be examined at the present time.

These impressive numbers shrink into a smaller compass when the total *light* of the stars is considered. It may readily be shown that if the formula for N_m is correct, the total intensity I_m of all the stars brighter than magnitude m can be represented by an expression identical in form with that for N_m; but whereas the peak for the error curve, the integral of which represents N_m or I_m, is in the former case (C = 23 or 24) beyond the limits of the observed data, in the case of I_m it is well within these limits—in fact, half the total light of the stars comes from those brighter than about 9.5^m. Up to this point the light received from all the stars of magnitude m to $m + 1$ increases; beyond this it diminishes rapidly, the increase in the number of the faint stars, great though it is, being insufficient to counterbalance their diminished brightness. Owing to the formula for N_m giving too small a number of bright stars (a defect of little moment for most values of m, in the case of N_m, but of serious importance when the total light of the stars is under consideration), the following table has been constructed from Table I, in order to give the actually observed light of the stars so far as magnitude 17, the formula being used only beyond this point, where it is quite sufficiently accurate for the purpose. The light is given in terms of the number of first magnitude stars of equivalent intensity. Three very bright stars are given individually.

TABLE II.—*The Equivalent Light of the Stars.*

Magnitude	Number	Equivalent number of 1st magnitude stars	Totals to magnitude m
– 1.6	Sirius	11	...
– 0.9	α Carinæ	6	...
– 0.0	α Centauri	2	...
m m			
0.0– 1.0	8	14	33
1.0– 2.0	27	17	50
2.0– 3.0	73	18	68
3.0– 4.0	189	19	87
4.0– 5.0	650	26	113
5.0– 6.0	2,200	35	148
6.0– 7.0	6,600	42	190
7.0– 8.0	22,550	56	246
8.0– 9.0	65,000	65	311
9.0–10.0	174,000	69	380
10.0–11.0	426,000	68	448
11.0–12.0	961,000	60	508
12.0–13.0	2,020,000	51	559
13.0–14.0	3,960,000	40	599
14.0–15.0	7,820,000	30	630
15.0–16.0	14,040,000	22	652
16.0–17.0	25,400,000	16	668
17.0–18.0	38,400,000	10	678
18.0–19.0	54,600,000	6	684
19.0–20.0	76,000,000	3	687
All stars fainter than 20·0...	0	3	690

It appears that the total light of the stars is approximately equal to that of 700 first magnitude stars. Previous estimates of this number have greatly erred on the side of excess (more than three times the present value having been given, though these estimates should be reduced by about 20 per cent for comparison with the present one, since they have been expressed in terms of first magnitude stars on the visual scale). The present value can scarcely be much affected by our ignorance as to the exact numbers of stars fainter than $17m$, as it is a fairly safe deduction from the above formulæ that the stars fainter than $15m$ contribute less than one eighth of the total light. Indeed, the fainter half of the stars, several hundred millions in number, account for only ¼ per cent of the total light, about equal to that of four second magnitude stars. It may be of interest, in conclusion, to express the total light of the stars in terms

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* Reproduced from *Nature*.

[1] Chapman and Melotte: The number of stars of each photographic magnitude down to 17.0m., in different galactic latitudes. Memoirs of the R.A.S., lx., 4.

Chapman: On the total light of the stars. Monthly notices of the R.A.S., lxxiv.

of the light of the full moon and of the standard candle; using some Harvard data for the brightness of these two sources of light, it appears that the full moon is very nearly one hundred times as bright as a star of magnitude —6.1, the light of which would equal the combined light of all the stars, while light of the same intensity would be received from an ordinary 16-candle-power electric lamp at forty-five or fifty yards distance.

Correspondence

[The editors are not responsible for statements made in the correspondence column. Anonymous communications cannot be considered, but the names of correspondents will be withheld when so desired.]

The Application of Carbon Dioxide Gas to the Soil

Beneficial Effects Upon Plant Growth Claimed

CERTAIN circumstances in my agricultural experience having suggested the idea of applying carbonic acid (CO_2) direct to the soil as a fertilizing agent, I put that gas into the ground in an experiment in my garden in two different ways with striking results.

First, I applied it for six months before planting while the soil was idle. This, to the end that the gas might have time, mingled with rain, to act as a solvent on fertilizing materials said to be lying inert, because undissolved, in practically all soils. I also supplied the gas to several kinds of plants while growing.

Everyone is familiar with the practice of stimulating the soil's productiveness by artificial additions of nitrogen, of everything, in fact, but the carbon part of plant nourishment. I thought it might be a good plan to increase the latter, at all events for the solvent properties which its compound with oxygen, CO_2, has in the ground.

My first experiment—putting CO_2 into the soil before planting—I looked upon as a shortened fallowing process. Having measured off two plots, No. 1 was spaded over in September, the gas being introduced by means of a quarter-inch perforated pipe. A length of the pipe was laid along the bottom of each trench as it was dug across the plot, and was there covered with earth from the next trench, some 8 or 9 inches deep.

The gas was generated in two small retorts, one on each end of the pipe, each having about a quart of dilute sulphuric acid on a handful of pulverized limestone; thus securing a carbonic acid atmosphere in the soil for the six months before planting any crop. Once a month I added a couple of teaspoonfuls of acid to the retorts and shook them up, so as to insure the continued supply of CO_2.

The adjoining plot, No. 2, was also spaded over the same depth on the same day. To this plot no gas was supplied, but, instead, a heavy dressing of well-rotted stable manure, which was thoroughly well mixed with the soil.

In the following Spring a crop test was made. The gas-treated plot, No. 1, was planted to potatoes without any other fertilizer. No. 2 was planted also with potatoes from the same bag on the same day, having also no further fertilizer. Both plots were cultivated exactly alike as the crops grew. The result was that when the crops were gathered and measured, almost 25 per cent more, and in every way better, potatoes were gathered from No. 1 plot than from No. 2.

As far as I could judge of this as a fallowing process, the six months' treatment of No. 1 plot with CO_2, as mentioned, was about equivalent to two years fallowing of land in the old way. Therefore, that use of carbonic acid in the soil to act as a solvent of locked-up fertility already there, is a plan of unquestionable importance to agriculture.

Applying the gas to the soil beneath growing plants also gave interesting results. This was done by sinking a perforated pipe in the soil directly underneath the roots of growing cabbages, Brussels sprouts and privet, and forcing in CO_2 daily for a week, then twice a week, for about a month. This greatly changed the size and appearance of these plants. They all grew taller, the cabbages and sprouts becoming coarser, their leaves toughened and leathery—looking almost like shrubs or young trees.

It is right to mention, however, that in the case of two of the cabbages which happened to have ample plant food (of what might be called the flesh-producing kinds) along with the gas, that these headed and were quite normal in quality while much larger than those not given gas. Again, while the leaves of the ordinary cabbages were considerably riddled in the usual way by slugs and other parasites, the gas-treated plants had no sign of these whatever.

This result of applying CO_2 to the soil around growing plants raises the question whether the nutritive effect was due to the solvent action of the gas on the otherwise inert fertilizing ingredients in the soil, or was it that the plants absorbed the CO_2 directly through their roots as nourishment? Doubtless some will view it one way and some another.

Prof. Liebig's "Complete Chemistry," under subheading "Chemistry in its Application to Agriculture and Physiology," page 19, last paragraph, says: "Plants during their life constantly possess the power of absorbing by their roots moisture, and, along with it, air and carbonic acid."

Storer's "Chemistry" (New York, 1899), Vol. II, page 220, mentions a certain condition of which it is said that it "seemed to indicate that all plants might take carbon from the soil on occasion." This same work, twelve years after my experiments, gives tabulated statements of the experiments of Stoeckheart and Peters (Storer's "Chemistry," Vol. II, page 180): "They filled several tall glass jars (2½ feet high by 5½ inches wide) with a rather poor loamy soil containing considerable humus, and they planted in each jar an equal number of seeds of peas and oats. Jar No. 1 was left to itself; that is to say, it was merely watered. Through the earth of jar No. 2 about 3½ pints of air were blown daily through a tube that reached to the bottom of the jar.

Through the earth of a third jar the same bulk of a mixture of air and one quarter CO_2 was forced, while through the earth of a fourth jar there was forced daily a mixture of one quarter CO_2, one quarter oxygen and half air. After three months and a half the crops were harvested, dried thoroughly and weighed. Their weights are given in grammes in following table:

	1	2	3	4
Oats	3.90	7.65	8.49	5.11
Peas	1.72	2.46	3.26	3.49
Roots of both	0.27	0.38	0.60	0.37
Total dry material	5.89	10.49	12.35	8.97
Ashes	0.52	0.95	1.12	1.01
If crop 1 equal 1	1.00	1.08	2.1	1.5

From this it is plain that jar No. 3 which had CO_2 alone with air blown through the soil (the nearest approach of any to my plan) is by far the most successful. But, of course, that mode of application of the gas would be entirely prohibitive in cost.

Dr. Parker McIlhiney, the well-known consulting chemist of New York, when asked what he thought of my discovery as to this new use of carbonic acid, replied that: "A claim in a matter of so great moment must not be condemned hastily. . . . It is always well to bear in mind that practically all great discoveries have been hit upon in just this way, by men working outside the established lines of experiment, and that experts have usually ridiculed such discoveries at the start. . . . There is a chance that this is a true discovery."

It seems as if tree cultivation stands as much in need of any advantages to be derived from this new method of fertilization as any other. In the year 1909 I became acquainted with Rev. B. V. D. Wycoff of Readington, Hunterdon County, N. J., who all his life has been a persistent tree planter, and who farms his own glebe. Showing me through his orchard, after saying that the common enemy, scale, was destroying all fruit trees in that section, his own among the rest, he called my attention to a curious kind of selection on the part of the scourge.

His orchard slopes quite sharply down from the parsonage into the valley. He pointed out two peach trees which he had planted with his own hands, about 60 feet apart, on the same day, some five or six years before. Both had done well, he said, for the first two or three years. Then one began to show signs of scale, and year after year grew worse, until, as we looked at it, there were only a few twigs left alive on one branch. The other, on the contrary, was a perfect wonder of blooming health. This great difference Mr. Wycoff was at a total loss to account for; he had puzzled over it many a time.

I suggested one thing after another, everything I could imagine—possibly a deeper hole for one than the other at planting, or different soil or subsoil, etc. No; the pastor was quite sure there was no such reason. "But stay!" he said at last, as something seemed to dawn on his mind; he did now recall something. And pointing to a little outhouse just outside the fence above us, explained that about two years after planting the twin trees he had had that vault cleaned out and the contents buried in a trench some four feet deep, a few feet up the slope, directly above the flourishing tree! At last we hit on the cause of the remarkable difference between the two trees. Cause and effect stood revealed. Nothing could be plainer.

I had already explained my contention as to scale and other parasites—that it never attacked a properly nourished tree. So, naturally, I claimed that the facts of the case before us were absolute proof of the correctness of my view. And Mr. Wycoff very heartily expressed his strong leaning now to my way of thinking; readily assenting to have anyone referred to him for confirmation of the details.

I then explained that at Mr. James Cypher's Black Oak farm, near the same village (a member of Mr. Wycoff's congregation), the only perfectly healthy fruit tree to be found was a vigorous cherry tree, which, laden with ample foliage and fruit, stood as an exception to general desolation from scale. And what was the cause here? The tree was located about ten feet from the fence of the farmyard. Underneath this fence the tree had sent a great root which reached out into the middle of the yard, branching out underneath several feet deep of fine black manure. That tree was perfect in every way, while the others, growing on what was known as the best of orchard soil, but never receiving any additional help, were all more or less victims of the scale.

Another case clearly illustrating the same important truth was that of a large apple tree at the east end of the house on the little Ellen Vanderveer farm at Milltown, on the North Branch River, in Somerset County. That being practically the only shade tree when I went there to live in 1910, I was sorry to see that it was far gone in disease, apparently given over to many kinds of parasite life, among them brown-tailed moths, several terrible nests of which clung to the branches. What leaves appeared were of an unhealthy whitish-blue color rather than green, and generally curling around larvæ of one kind or another.

I talked with Henry Carkeuff, then the farmer opposite, about trying to save the tree. He thought it would be hopeless to attempt it; he had lived there nine years and in that time the tree had never borne his hatful of apples, and they were little, wooden things as sour as vinegar. I determined, however, to try my new treatment and see what would happen.

It was late in April, so not much could be done for that season. But I broke off several dead branches, dug among the roots and applied carbonic acid to the soil, with occasionally a few pailfuls of well water to wash it in, using a pickax and even a crowbar. I loosened the earth so the gas could sink well down into it. The gas was put into the soil, as much as possible along the main roots, by means of a perforated composition pipe, which was covered generally some five or six inches deep. There was soon plain evidence of improvement. Mr. Carkeuff noticed it and spoke of it during the Summer; there were also about two bushels of apples that season. The treatment was continued through Summer and Fall; sometimes—not more than twice or thrice—a couple of pailfuls of waste water from the farmyard were also flung on the roots. In the Fall I increased the CO_2 until frost came. In the following Spring the treatment was resumed. I used two half-gallon glass jars as retorts; in these were used about a quarter of a bushel of broken marble and pulverized limestone, altogether, and not more than three quarts of sulphuric acid. The latter was used, of course, much diluted, a small quantity to a charge, and a few handfuls of the carbonates at a time.

With the Spring of 1911 came something to look at! For that formerly dying old tree burst forth in such a blaze of blossom that the neighbors passing by stopped their horses to gaze at it. And as to fruit, when the time came, besides what was eaten by the horse and chickens—bushels upon bushels of windfalls—there were gathered 68 half-bushel basketfuls (heaping full) of as fine, rosy-cheeked, sound apples, and as good cookers as could be desired.

After long years as a typical case of decay from scale, moths, etc., that old tree, measuring about two feet in diameter of trunk, through receiving such nourishment as it required, in one year became the talk of the neighborhood as a beautiful, healthy and fruitful tree that was a pleasure to look upon.

From these facts everyone will draw his own conclusions. The salient point is plain enough, that because these trees were properly nourished parasite germs (which infested other nearby trees) were powerless to harm them. In two cases, natural, organic manures supplied the necessary shield from disease; while in the third case, carbonic acid and water completely transformed a diseased, moth-infested apple tree into renewed fruitfulness with apparently a new lease of life. The tree is there still, and though the remedial treatment ceased four years ago, I am told it flourishes still on the strength of it.

New York. GEORGE QUARRIE.

How Bodies Fall in Deep Vertical Shafts*

By F. W. McNair

THE mystery of the dark has always hung over caverns, whether of Nature's excavation or of the digging of venturesome man. Especially is this so when they attain great extent or great depth. Instinctively one feels that

* Reproduced from the *Mining and Scientific Press*.

in a very deep shaft or far underground, conditions must be quite different from those one finds on the surface, and, perhaps because of the one obvious and striking difference due to the absence of light, there is a tendency to expect the uncanny. When, therefore, there is made the statement that objects dropped into the deep shafts of the Lake Superior copper district "do not fall to the bottom, but are invariably found clinging to the east side of the shaft," probably the average reader is apt to credit the statement as noting one more of the mysterious facts which belong to the underworld, without any serious attempt to inquire what may be the truth of the matter. When the press correspondent, in telling it, adds a touch of the gruesome in the implication that among the objects frequently found "clinging to the east side of the shaft" are the pieces of a dismembered human body, one may suppose that reportorial skill has reached its limit, and that there has been produced a real gem of a newspaper story. Such a story was put forth not long ago by an enterprising correspondent and was copied in the daily press throughout the length and breadth of the country. One may question whether a plain statement of the truth will obtain more than a fraction of the same extent of circulation.

Yet the truth, though it contains, of course, nothing of the supernatural and little indeed of the unfamiliar, is nevertheless sufficiently interesting to make it worth while to narrate the facts that lie behind the statement that objects dropped into the deep vertical shafts do not fall to the bottom.

To begin with, this statement in itself is much more nearly exact than the thoughtful person would at first be inclined to believe. Very rarely, to say the least, do objects falling from the collar reach the bottom of the shaft, nor, indeed, anywhere near it. On investigation, this fact is not so very surprising nor are most of the reasons for it so far to seek. The vertical shafts of the Lake Superior copper district are very deep, some of them the deepest in the world. Further, *in proportion to their depth* they are of small cross-section. The common impression seems to be in contradiction to this last statement, and at first sight when we learn that a shaft like No. 5 of the Tamarack mine is 9 by 30 feet in cross-section, its horizontal dimensions seem little short of colossal. But if it is noted that at 5,000 feet the shaft is over 500 times as deep as it is wide, the horizontal dimensions begin to look small.

Moreover, it must be remembered that the shaft is divided by the timbering throughout its depth into compartments, the larger of which are roughly 5 by 7 feet.[1]

Really then in the deepest shafts the separate compartments form tubes more than 700 times as deep as the greater horizontal measurement, and over 1,000 times as deep as the smaller dimension. In proportion, they do not differ greatly from the tube of the ordinary chemical thermometer. My experience is that few persons even among those somewhat familiar with such shafts realize or comprehend these proportions. This is undoubtedly because to a person in it so little of the length of the shaft is visible at one time. Taking into account the relatively extreme smallness of cross-section, it is easy to see that an object falling into the shaft must follow a straight line almost exactly parallel with the axis of the compartment, if it is not to hit a wall in its downward course.

If the walls were smooth, the object might strike many times, and, simply rebounding, keep on its downward way. But in a shaft timbered in the ordinary manner, striking the walls means a lodgment sooner or later, and consequent interruption of the fall. In the Tamarack No. 5 shaft, the length of the set from center to center is 6 feet.[2] Back of these timbers there is either an irregular rock surface or the irregular surface of the lagging. There are, therefore, more than 800 opportunities for an object striking an outside wall on its way down to find lodgment.

Were there no air present, a body would require more than 17½ seconds to fall 5,000 feet. The air in the shaft, denser than at surface, increases the time occupied in descent. Under such conditions, it is clear that the horizontal velocity permissible, if the body is to reach the bottom, is small indeed. Less than one half a foot per second would carry it clear across the longer horizontal dimension while it was falling. Objects dropping into the shaft under ordinary conditions nearly always start with some horizontal velocity, indeed it is usually due to such initial velocity in the horizontal that they get into the shaft at all.

Almost all common objects are irregular in shape, and, drop one of them ever so carefully, contact with the air through which it is passing soon deviates it from the vertical, giving it a horizontal velocity, and this when the air is quite still. The object slides one way or another on the air it compresses in front of it. Even if the body is a sphere, the air will cause it to deviate, if it is rotating about an axis out of the vertical. Again, the air in the shaft is in ceaseless motion, and any obliquity of the currents would obviously deviate the falling body from the vertical, no matter what its shape. If the falling object is of iron or steel, the magnetic influence of the air and steam-mains which pass down the shaft, and which invariably become strongly magnetic, may cause it to swerve from a vertical course.

The hindrances to describing a vertical path so far considered are well within familiarity for the person of average information, but there is another cause of deviation the action of which is not so familiar, and which yet operates to carry a falling body to one side, always in a definite direction. The cause itself is familiar enough, being nothing other than the diurnal rotation of the earth on its axis. Because of this rotation, a body dropped from a fixed position or projected vertically downward will, if then left entirely to itself, bring up always to the eastward of the vertical along which it would otherwise fall. It might seem at first thought as if the body should depart from the vertical to the westward rather than to the eastward, since the earth rotates in the latter direction. However, the so-called fixed position is fixed only with reference to the earth's surface, and the object before release partakes of the same motion, and has the same velocity, as all other "fixed" objects in its neighborhood. Were it at the equator, this speed would be, say, a thousand miles per hour. The speed grows less toward the poles, where it becomes nothing. In fact, the object is traveling in the circumference of the circle of latitude in which it happens to lie. Points beneath it have a speed which is smaller as they lie nearer the axis of rotation. The freely falling body retains the eastward speed with which it started, and so gains on the slower moving parts of the earth which it is approaching. It, therefore, moves eastward from the vertical in which its fall began. The reader who in his study of physical geography worked over the reason of the easterly or westerly deviation of the trade winds was considering an exactly similar set of conditions.

This easterly deviation was pointed out by Newton over two centuries ago, who stated in the quaint language of the day that the falling body "will not descend the perpendicular, . . . but outrunning the parts of the earth will shoot forward to the east side of the perpendicular. . ." It has therefore been a fact well known to physicists for a long time.

During investigations of the cause of the divergence of long plumb-lines hung in No. 5 shaft of the Tamarack mine,[4] some of the mining people called attention to "the old Cornish method" of plumbing a shaft by dropping a suitable object like a spherical shot from a determined point of suspension at the collar of the shaft and receiving it in a prepared bed of clay at the bottom, the vertical being then assumed as the line joining the point of suspension with the center of the depression made in the clay. This suggestion led those engaged in the investigation to discuss the amount of easterly deviation which would be observed if a sphere were dropped 4,200 feet, the length of the plumb-line then under observation. A rough calculation by one of the party gave something like four feet. A deviation which could be consistently measured in feet excited great interest and everyone concerned expressed a desire to witness a trial by experiment of so striking a demonstration of the earth's diurnal rotation.

Accordingly, at the end of a certain day's plumbing operations a steel sphere, chosen because it was the only convenient object at hand, was suspended about one foot from the timbers near the western corner of the compartment. The compartment stands diagonally with reference to the cardinal points. Forty-two hundred feet below a clay bed was placed, having its eastern edge some five feet east of the point of suspension of the ball. When the ball appeared to be still the suspending thread was burned, and the instant of the dropping of the ball was indicated by a prearranged signal transmitted by telephone to the observers below, who, watch in hand, waited for the sphere to strike the bed of clay. It failed to appear at all. Another like sphere was hung in the center of the compartment and the trial was repeated with the same result. The shaft had to be cleared and no more trials were feasible. Some months after, one of the spheres, presumably the latter one, was found by a timberman where it had lodged in the timbers 800 feet from surface.

It is, however, not probable that these balls lodged because of the earth's rotation alone. The rough calculation mentioned above gave too large a figure for the deviation. The matter is really more complicated than the foregoing discussion implies. It has received mathematical treatment from the great Gauss. According to his results, the deviation to the east for a fall of 5,000 feet at the Tamarack mine should be a little under three feet. Both spheres had that much to spare before striking the timbers. It is almost certain, therefore, that others of the causes mentioned in the beginning acted to prevent a vertical fall. At any rate, these trials serve to emphasize the unlikelihood that an object which falls into a deep vertical shaft, like those at the Tamarack mine, will reach the bottom, even when some care is taken in selecting it and also to start it vertically. No account of the trials was published because they lacked serious purpose too much and were too crude to be dignified as real experiments, but they excited great interest among the people about the mine and talk about them spread until it finally gave foundation for the exaggerated press story of objects "clinging to the east side of the shaft."

If the timbering permits lodgment, as is the case in most shafts, it may truthfully be said that if a shaft is deep in proportion to its cross-section few indeed will be the objects falling into it which will reach the bottom, and such objects are more likely to lodge on the easterly side than on any other.

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[1] See "The Copper Mines of Lake Superior," by T. A. Rickard, p. 93.

[2] See "No. 5 Shaft at the Tamarack Mine," by W. E. Parnall, Proceedings Lake Superior Mining Institute, 1901.

[4] Quoted by Prof. E. H. Hall, *Physical Review*, vol. XVII, p. 180, September, 1903.

[4] *Electrical World*, April 26th, 1902, also *Science*, June 20th, 1902, or *Terrestrial Magnetism*, September, 1902.

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